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PHYSIK UND REALITÄT.

VON

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§ 1. ALLGEMEINES ÜBER DIE WISSENSCHAFTLICHE METHODE.

Oft und gewiss nicht ohne Berechtigung ist gesagt worden, dass der Naturwissenschaftler ein schlechter Philosoph sei. Warum sollte es also nicht auch für den Physiker das Richtigste sein, das Philosophieren dem Philosophen zu überlassen? Für eine Zeit, in welcher die Physiker über ein festes, nicht angezweifeltes System von Fundamentalbegriffen und Fundamentalgesetzen zu verfügen glauben, mag dies wohl so sein, nicht aber in einer Zeit, in welcher das ganze Fundament der Physik problematisch geworden ist, wie gegenwärtig. In solcher Zeit des durch die Erfahrung erzwungenen Suchens nach einer neuen, solideren Basis kann der Physiker die kritische Betrachtung der Grundlagen nicht einfach der Philosophie überlassen, weil nur er selber am besten weiss und fühlt, wo ihn der Schuh drückt; auf der Suche nach einem neuen Fundament muss er sich über die Berechtigung bezw. Notwendigkeit der von ihm benutzten Begriffe nach Kräften klar zu werden versuchen.

Alle Wissenschaft ist nur eine Verfeinerung des Denkens des Alltags. Damit hängt es zusammen, dass die kritische Besinnung des Physikers sich nicht auf die Untersuchung der

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Begriffe seiner besonderen Wissenschaft beschränken kann, sondern dass er an der kritischen Betrachtung des viel schwieriger zu analysierenden Denkens des Alltags nicht achtlos vorbeigehen kann.

Auf der Bühne unseres seelischen Erlebens erscheinen in bunter Folge Sinneserlebnisse, Erinnerungsbilder an solche, Vorstellungen und Gefühle. Im Gegensatz zur Psychologie beschäftigt sich die Physik (unmittelbar) nur mit den Sinneserlebnissen und dem "Begreifen" des Zusammenhanges zwischen ihnen. Aber auch der Begriff der "realen Aussenwelt" des Alltagsdenkens stützt sich ausschliesslich auf die Sinneseindrücke.

Nun ist zunächst zu bemerken, dass uns die Unterscheidung zwischen Sinneseindrücken (Empfindungen) und Vorstellungen nicht oder doch nicht mit Sicherheit gegeben ist. Mit dieser Problematik, welche auch den Realitätsbegriff affiziert, wollen wir uns hier aber nicht beschäftigen, sondern die Sinneserlebnisse als solche, bezw. als seelische Erlebnisse besonderer Art erkennbar und gegeben annehmen.

Der erste Schritt zur Setzung einer "realen Aussenwelt" liegt nach meiner Ansicht in der Bildung des Begriffes des körperlichen Objekts, bezw. körperlicher Objekte verschiedener Art. Gewisse sich wiederholende Komplexe von Sinnesempfindungen (zum Teil zusammen mit Sinnesempfindungen, die als Zeichen für Sinneserlebnisse der Mitmenschen gedeutet werden) werden gedanklich aus der Fülle des Sinneserlebens willkürlich herausgehoben, und es wird ihnen ein Begriff zugeordnet—der Begriff des körperlichen Objekts. Logisch betrachtet ist dieser Begriff nicht identisch mit der Gesamtheit jener Sinnesempfindungen, sondern er ist eine freie Schöpfung des menschlichen (oder tierischen) Geistes. Dieser Begriff verdankt aber andererseits seine Bedeutung und Berechtigung ausschliesslich der Gesamtheit jener Sinnesempfindungen, denen er zugeordnet ist.

Der zweite Schritt liegt darin, dass wir jenem Begriff des körperlichen Objektes in unserem (unsere Erwartungen bestimmenden) Denken von den jenen Begriff veranlassenden Sinnesempfindungen weitgehend unabhängige Bedeutung zuschreiben. Dies meinen wir, wenn wir dem körperlichen Objekt "reale Existenz" zuschreiben. Die Berechtigung

dieser Setzung liegt einzig darin, dass wir mit Hilfe derartiger Begriffe und zwischen ihnen gesetzter gedanklicher Relationen uns in dem Gewirre der Sinnesempfindungen zurecht zu finden vermögen. Damit hängt es zusammen, dass jene Begriffe und Relationen—obgleich freie Setzungen des Denkens—uns fester und unabänderlicher erscheinen als das einzelne Sinneserlebnis, dessen Charakter der Illusion oder Hallucination gegenüber doch nie vollkommen gesichert erscheint. Andererseits aber haben jene Begriffe und Relationen, insbesondere die Setzung realer Objekte, überhaupt einer "realen Welt," nur insoweit Berechtigung, als sie mit Sinneserlebnissen verknüpft sind, zwischen welchen sie gedankliche Verknüpfungen schaffen.

Dass die Gesamtheit der Sinneserlebnisse so beschaffen ist, dass sie durch das Denken (Operieren mit Begriffen und Schaffung und Anwendung bestimmter funktioneller Verknüpfungen zwischen diesen sowie Zuordnung der Sinneserlebnisse zu den Begriffen) geordnet werden können, ist eine Tatsache, über die wir nur staunen, die wir aber niemals werden begreifen können. Man kann sagen: Das ewig Unbegreifliche an der Welt ist ihre Begreiflichkeit. Dass die Setzung einer realen Aussenwelt ohne jene Begreiflichkeit sinnlos wäre, ist eine der grossen Erkenntnisse Immanuel Kants.

Wenn hier von "Begreiflichkeit" die Rede ist, so ist dieser Ausdruck hier zunächst in seiner bescheidensten Bedeutung gemeint. Er bedeutet: durch Schaffung allgemeiner Begriffe und Beziehungen zwischen diesen Begriffen, sowie durch irgendwie festgelegte Beziehungen zwischen Begriffen und Sinneserlebnissen zwischen letzteren irgend eine Ordnung herstellen. In diesem Sinne ist die Welt unserer Sinneserlebnissen begreifbar, und dass sie es ist, ist ein Wunder.

Ueber die Art und Weise, wie wir Begriffe zu bilden und zu verknüpfen haben, und wie wir sie den Sinneserlebnissen zuzuordnen haben, lässt sich nach meiner Ansicht a priori nicht das Geringste aussagen. Nur der Erfolg bezüglich der Herstellung einer Ordnung der Sinneserlebnisse entscheidet. Die Regeln der Verknüpfungen von Begriffen müssen nur überhaupt festgelegt sein, da sonst Erkenntnis in dem von uns angestrebten Sinne unmöglich wäre. Man hat diese Regeln mit den Regeln eines Spieles verglichen, die an sich willkürlich

sind, deren Bestimmtheit aber das Spiel erst möglich macht. Diese Festlegung wird aber niemals eine endgültige sein können, sondern nur für einen ins Auge gefassten Anwendungsbereich Gültigkeit beanspruchen dürfen, (d.h. es gibt keine endgültigen Kategorien im Sinne Kants.)

Die Verknüpfung der elementaren Begriffe des Alltags-Denkens mit Komplexen von Sinneserlebnissen ist nur intuitiv erfassbar und wissenschaftlich logischer Fixierung unzugänglich. Die Gesamtheit dieser Verknüpfungen—selbst nicht begrifflich fassbar—ist das einzige, was das Gebäude der Wissenschaft von einem leeren logischen Begriffs-Schema unterscheidet; vermöge dieser Verknüpfungen werden die rein begrifflichen Sätze der Wissenschaft zu generellen Aussagen über Komplexe von Sinneserlebnissen.

Die mit typischen Komplexen von Sinneserlebnissen direkt und intuitiv verknüpften Begriffe wollen wir "primäre Begriffe" nennen. Alle andern Begriffe sind—physikalisch betrachtet—nur insoweit sinnvoll, als sie mit den "primären Begriffen" durch Sätze in Verbindung gebracht sind. Diese Sätze sind teils Definitionen der Begriffe (und hieraus logisch ableitbarer Aussagen), teils Sätze, die nicht aus den Definitionen zu folgern sind und wenigstens indirekt Beziehungen zwischen den "primären Begriffen" und damit zwischen Sinneserlebnissen aussagen. Sätze der letzteren Art sind "Behauptungen über die Wirklichkeit" oder "Naturgesetze," d.h. Sätze, die sich zu bewähren haben an den durch die primären Begriffe erfassten Sinneserlebnissen. Welche von den Sätzen als Definitionen und welche als Naturgesetze anzusprechen sind, hängt weitgehend von der gewählten Darstellung ab; es ist überhaupt nur dann notwendig, eine solche Unterscheidung wirklich durchzuführen, wenn man untersuchen will, inwieweit das ganze ins Auge gefasste Begriffssystem vom physikalischen Standpunkte betrachtet wirklich inhaltvoll ist.

Schichtenstruktur des Wissenschaftlichen Systems.

Ziel der Wissenschaft ist erstens die möglichst *vollständige* begriffliche Erfassung und Verknüpfung der Sinneserlebnisse in ihrer ganzen Mannigfaltigkeit, zweitens aber die Erreichung dieses Zieles *unter Verwendung eines Minimums von primären*

Begriffen und Relationen (Streben nach möglichster logischer Einheitlichkeit des Weltbildes bezw. logischer Einfachheit seiner Grundlagen).

Die Wissenschaft braucht die ganze Mannigfaltigkeit der primären, d.h. unmittelbar mit Sinneserlebnissen verknüpften Begriffe sowie der sie verknüpfenden Sätze. In ihrem ersten Entwicklungsstadium enthält sie nichts weiter. Auch das Denken des Alltags begnügt sich im grossen Ganzen mit dieser Stufe. Diese kann aber einen wirklich wissenschaftlich eingestellten Geist nicht befriedigen, da die so gewinnbare Gesamtheit von Begriffen und Relationen der logischen Einheitlichkeit völlig entbehrt. Um diesem Mangel abzuhelfen, erfindet man ein begriffs- und relationsärmeres System, welches die primären Begriffe und Relationen der "ersten Schicht" als logisch abgeleitete Begriffe und Relationen enthält. Dieses neue "sekundäre System" erkauft die gewonnene höhere logische Einheitlichkeit mit dem Umstande, dass seine an den Anfang gestellten Begriffe (Begriffe der zweiten Schicht) nicht mehr unmittelbar mit Komplexen von Sinneserlebnissen verbunden sind. Weiteres Streben nach logischer Einheitlichkeit führt zur Aufstellung eines noch ärmeren tertiären Systems von Begriffen und Relationen zur Deduktion der Begriffe und Relationen der sekundären (und damit indirekt der primären) Schicht. So geht es fort, bis wir zu einem System von denkbar grösster Einheitlichkeit und Begriffsarmut der logischen Grundlagen gelangt sind, das mit der Beschaffenheit des sinnlich Gegebenen vereinbar ist. Ob wir bei diesem Streben je zu einem definitiven System kommen, wissen wir nicht. Wird man um seine Meinung gefragt, so ist man geneigt, mit Nein zu antworten; beim Ringen mit den Problemen wird man aber wohl von der Hoffnung getragen, dass dies höchste Ziel wirklich weitgehend erreichbar sei.

Ein Anhänger der Abstraktions- bezw. Induktions-Theorie würde die vorgenannten Schichten "Abstraktions-Stufen" nennen. Ich halte es aber für unrichtig, die logische Unabhängigkeit der Begriffe gegenüber den Sinneserlebnissen zu verschleiern; es handelt sich nicht um eine Beziehung wie die der Suppe zum Rindfleisch, sondern eher wie die der Garderobe-Nummer zum Mantel.

Ferner sind die Schichten nicht klar gegeneinander abge-

grenzt. Nicht einmal die Zugehörigkeit eines Begriffes zur primären Schicht ist völlig scharf. Es handelt sich hierbei eben um freigebildete Begriffe, die mit einer für die Anwendung hinreichenden Sicherheit mit Komplexen von Sinneserlebnissen intuitiv verknüpft sind, so dass bei dem Konstatieren des Zutreffens oder Nicht-Zutreffens eines Satzes für einen besonderen Erlebnisfall (Experiment) keine Unsicherheit besteht. Wesentlich ist nur die Bestrebung, die Vielheit der erlebnisnahen Begriffe und Sätze als logisch abgeleitete Sätze einer möglichst engen Basis von Grund-Begriffen und Grund-Relationen darzustellen, die ihrerseits an sich frei wählbar sind (Axiome). Mit dieser Freiheit ist es aber nicht weit her; sie ist nicht ähnlich der Freiheit eines Novellen-Dichters, sondern vielmehr der Freiheit eines Menschen, dem ein gut gestelltes Worträtsel aufgegeben ist. Er kann zwar jedes Wort als Lösung vorschlagen, aber es gibt wohl nur *eines*, welches das Rätsel in allen Teilen wirklich auflöst. Dass die Natur—so wie sie unseren Sinnen zugänglich ist—den Charakter eines solchen gut gestellten Rätsels habe, ist ein Glaube, zu welchem die bisherigen Erfolge der Wissenschaft allerdings einigermassen ermutigen.

Die Vielheit der Schichten, von der wir oben gesprochen haben, entspricht den einzelnen Fortschritten, die das Ringen um Einheitlichkeit der Basis im Laufe der Entwicklung gezeitigt hat. Vom Standpunkte des Endzieles sind die intermediären Schichten nur temporär, um am Ende als belanglos zu verschwinden. Wir aber haben es zu tun mit der Wissenschaft von heute, in welcher diese Schichten problematische Teilerfolge darstellen, welche sich gegenseitig stützen, aber auch gegenseitig bedrohen; denn das Begriffssystem von heute weist tiefe Zwiespältigkeiten auf, auf die wir später stossen werden.

Das Ziel der folgenden Zeilen ist es, zu zeigen, welche Wege der konstruierende Menschengeist eingeschlagen hat, um zu einer logisch möglichst einheitlichen begrifflichen Basis der Physik zu gelangen.

§ 2. DIE MECHANIK UND DER VERSUCH, AUF SIE DIE GESAMTE PHYSIK ZU GRÜNDEN.

Eine wichtige Eigenschaft unserer Sinneserlebnisse wie unserer Erlebnisse überhaupt ist ihre zeitliche Ordnung.

Diese Ordnungs-Eigenschaft führt zur gedanklichen Konstruktion der subjektiven Zeit, eines Ordnungs-Schemas für die Erlebnisse. Die subjektive Zeit führt dann über den Begriff des körperlichen Objekts und des Raumes zu dem der objektiven Zeit, wie wir später sehen werden.

Dem Begriffe der objektiven Zeit geht aber der des Raumes voraus, und diesem der Begriff des körperlichen Objektes; letzterer ist direkt mit Komplexen von Sinneserlebnissen verknüpft. Es wurde als charakteristische Eigenschaft des Begriffes "körperliches Objekt" bezeichnet, dass wir letzterem eine Existenz zuordnen, unabhängig von der ("subjektiven") Zeit und unabhängig von seiner sinnlichen Wahrnehmung. Wir tun dies, obwohl wir an ihm zeitliche Aenderungen wahrnehmen. Poincaré hat nun mit Recht hervorgehoben, dass wir am körperlichen Objekte zwei Arten Aenderungen unterscheiden, nämlich "Aenderungen des Zustandes" und "Aenderungen der Lage"; letztere seien solche Aenderungen, welche wir durch willkürliche Bewegungen unseres Körpers rückgängig machen können.

Dass es körperliche Objekte gibt, denen wir innerhalb eines gewissen Wahrnehmungsgebietes keine Aenderungen des Zustandes, sondern nur solche der Lage zuzuschreiben haben, ist für die Bildung des Raumbegriffes fundamental wichtig, (in gewissem Masse sogar für die Berechtigung des Begriffes des körperlichen Objektes); wir wollen ein solches Objekt "praktisch-starr" nennen.

Wenn wir zwei praktisch-starre Körper gleichzeitig, d.h. als ein Ganzes, zum Objekt unserer Sinneswahrnehmung machen, so gibt es für dies Ganze solche Aenderungen, welche *nicht* als Aenderungen der Lage für das Ganze aufgefasst werden können, obwohl dies für jeden der beiden Konstituenten der Fall ist. Dies führt zu dem Begriff der "Aenderung der relativen Lage" der beiden Objekte und damit auch zu dem Begriff der "relativen Lage" der beiden Objekte. Es erweist sich ferner, dass es unter den relativen Lagen eine besondere Art gibt, die wir als "Berührung" bezeichnen.[1]

[1] Es liegt in der Natur der Sache, dass wir von diesen Gegenständen nur mittelst der von uns gebildeten Begriffe sprechen können, die selbst keiner Definition zugänglich sind. Wesentlich ist aber, dass wir nur solche Begriffe verwenden, über deren Zuordnung zum Erlebnis-Material wir uns sicher fühlen dürfen.

Dauernde Berührung zweier Körper in drei oder mehr "Punkten" bedeutet deren Vereinigung zu einem (quasi-starren) Gesamtkörper. Man kann sagen, der erste Körper werde dann quasi-starr durch den zweiten Körper fortgesetzt, welcher seinerseits wieder quasi-starr fortgesetzt werden kann. Die quasi-starre Fortsetzbarkeit eines Körpers ist unbegrenzt. Der Inbegriff der denkbaren quasi-starren Fortsetzungen eines Körpers K_0 is der durch ihn bestimmte unendliche "Raum."

Nach meiner Ansicht ist der Umstand, dass jeder irgendwie gelagerte Körper mit der quasi-starren Fortsetzung eines bestimmten gewählten Körpers K_0 (Bezugskörper) in Berührung gebracht werden kann, die empirische Basis unseres Raumbegriffes. Im vorwissenschaftlichen Denken spielt die feste Erdkruste die Rolle von K_0 und seiner Fortsetzung. Die Bezeichnung Geometrie deutet darauf hin, dass der Raumbegriff psychologisch mit der Gegebenheit des Erdkörpers zusammenhängt.

Die kühne Begriffsbildung "Raum," welche aller wissenschaftlichen Geometrie voranging, verwandelte gedanklich den Inbegriff der Lagenbeziehungen körperlicher Objekte in den Inbegriff der Lagen der körperlichen Objekte "im Raume." Dies bedeutet an sich schon eine grosse formale Vereinfachung. Durch sie wird auch erreicht, dass jede Lagen-Aussage implizite eine Berührungs-Aussage ist; ein Punkt eines körperlichen Objektes befindet sich im Raumpunkte P bedeutet: das Objekt berührt mit dem ins Auge gefassten Punkte den Punkt P des (passend fortgesetzt gedachten) Bezugskörpers K_0.

In der Geometrie der Griechen spielt der Raum nur eine sozusagen qualitative Rolle, indem die Lage der Objekte zu ihm zwar festgelegt gedacht wird, aber nicht durch Zahlen beschrieben wird. Letzteres hat erst Descartes unternommen. In seiner Sprechweise kann der ganze Inhalt der euklidischen Geometrie durch folgende Aussagen axiomatisch fundiert werden. (1) Zwei markierte Punkte eines starren Körpers bestimmen eine Strecke. (2) Den Punkten des Raumes lassen sich Zahlentripel X_1, X_2, X_3, so zuordnen, dass für jede ins Auge gefasste Strecke $P' - P''$ mit den Koordinaten ihrer Endpunkte $(X_1', X_2', \ldots, X_1'', X_2'', X_3'')$ der Ausdruck

$$s^2 = (X_1'' - X_1')^2 + (X_2'' - X_2')^2 + (X_3'' - X_3')^2$$

unabhängig ist von der Lage des Körpers und aller andern Körper. Die (positive) Zahl s heisst die Länge der Strecke oder der Abstand der beiden Raumpunkte P' und P''. (die mit den Punkten P' und P'' der Strecke koinzidieren.)

Die Formulierung ist absichtlich so gewählt, dass sie nicht nur den logisch-axiomatischen sondern auch den empirischen Gehalt der euklidischen Geometrie klar hervortreten lässt. Die rein logische (axiomatische) Darstellung der euklidischen Geometrie hat zwar demgegenüber den Vorteil grösserer Einfachheit und Klarheit. Sie erkauft aber diesen Vorteil durch den Verzicht auf die Darstellung des Zusammenhanges zwischen der begrifflichen Konstruktion und der sinnlichen Erfahrung, auf welchem Zusammenhang allein doch die Bedeutung der Geometrie für die Physik beruht. Der verhängnisvolle Irrtum, dass der euklidischen Geometrie und dem zugehörigen Raumbegriffe eine aller Erfahrung vorangehende Denknotwendigkeit zugrunde liege, beruhte darauf, dass die empirische Basis, welche der axiomatischen Konstruktion der euklidischen Geometrie zugrunde liegt, in Vergessenheit geraten war.

Insoweit man von der Existenz starrer Körper in der Natur sprechen kann, ist die euklidische Geometrie eine physikalische Wissenschaft, die sich an der Sinneserfahrung zu bewähren hat. Sie betrifft die Gesamtheit der Sätze, die für die zeitunabhängige relative Lagerung starrer Körper gelten sollen. Wie man sieht, ist auch der physikalische Raumbegriff, wie er ursprünglich in der Physik verwendet wurde, an die Existenz starrer Körper gebunden.

Die zentrale Bedeutung der euklidischen Geometrie vom physikalischen Standpunkte aus liegt darin, dass ihre Aussagen unabhängig von der besonderen Natur der Körper Gültigkeit beanspruchen, um deren relative Lagerung es sich handelt. Ihre formale Einfachheit ist durch die Eigenschaften Homogenität, Isotropie (und Existenz ähnlicher Gebilde) charakterisiert.

Der Raumbegriff ist für die eigentliche Geometrie, d.h. für die Formulierung der Gesetzmässigkeiten der relativen Lagerung starrer Körper, zwar nützlich aber nicht unentbehrlich. Dagegen ist der Begriff der objektiven Zeit, ohne welchen eine

Formulierung der Grundlagen der klassischen Mechanik nicht möglich ist, an den des räumlichen Kontinuums gebunden.

Die Einführung der objektiven Zeit besteht aus zwei voneinander unabhängigen Setzungen:

(1) Einführung der objektiven Lokalzeit, indem man den zeitlichen Ablauf des Erlebens in Verbindung bringt mit den Angaben einer "Uhr," d.h. eines abgeschlossenen Systems mit periodischem Ablauf.

(2) Einführung des Begriffes der objektiven Zeit für das Geschehene im *ganzen Raume*, durch welchen der Begriff der Localzeit erst zu dem Begriff der Zeit der Physik erweitert wird.

Bemerkung zu (1). Es bedeutet nach meiner Meinung keine petitio principii, wenn der Begriff des periodischen Ablaufes dem der Zeit vorangestellt wird, wenn es sich um die Aufhellung der Entstehung bezw. des empirischen Inhaltes des Zeitbegriffes handelt. Diese Auffassung entspricht durchaus dem Vorangehen des Begriffes des starren (bezw. quasi-starren) Körpers bei der Deutung des Raumbegriffes.

Nähere Ausführung zu (2). Die bis zur Aufstellung der Relativitäts-Theorie herrschende Illusion, es sei vom Erlebnisstandpunkte a priori klar, was Gleichzeitigkeit in Bezug auf räumlich distantes Geschehen und damit physikalische Zeit überhaupt bedeute, hat ihren Grund darin, dass wir in der Alltags-Erfahrung die Ausbreitungszeit des Lichtes vernachlässigen können. Wir pflegen daher "gleichzeitig sehen" und "gleichzeitig geschehen" nicht zu unterscheiden, wodurch der Unterschied zwischen Zeit und Lokalzeit verwischt wird.

Die Unschärfe, welche dem Zeitbegriffe der klassischen Mechanik vom Gesichtspunkte der empirischen Bedeutung anhaftet, wurde durch die axiomatischen Darstellungen dadurch verdeckt, dass sie Raum und Zeit als ein unabhängig von den Sinneserlebnissen Gegebenes behandelten. Solche "Hypostasierung" (Verselbstständigung) von Begriffen gereicht nicht notwendig der Wissenschaft zum Nachteil, es ensteht aber leicht der Irrtum, solche Begriffe, deren Ursprung

in Vergessenheit geraten ist, für denknotwendig und damit für nicht veränderbar unzusehen, was zu einer ernsten Gefahr für den Fortschritt der Wissenschaft werden kann.

Für die Entwicklung der Mechanik und darum für die Entwicklung der Physik überhaupt war es ein Glück, dass den früheren Denkern die dem Begriffe der objektiven Zeit anhaftende Unschärfe bezüglich seiner empirischen Deutung verborgen geblieben ist. Im vollen Vertrauen auf die reale Bedeutung der Raum-Zeit-Konstruktion entwickelte sich das Fundament der Mechanik, das wie folgt charakterisiert werden kann.

(a) Begriff des materiellen Punktes: körperliches Objekt, das hinsichtlich seiner Lage und Bewegung hinreichend genau als Punkt mit den Koordinaten X_1, X_2, X_3, beschrieben werden kann. Beschreibung von dessen Bewegung (inbezug auf den "Raum" K_0), indem X_1, X_2, X_3 als Funktionen der Zeit gegeben werden.

(b) Trägheitssatz: Verschwinden der Komponenten der Beschleunigung für einen materiellen Punkt, der von allen andern hinreichend weit entfernt ist.

(c) Bewegungsgesetz (für den materiellen Punkt): Kraft = Masse × Beschleunigung.

(d) Kraftgesetze (Wechselwirkungen zwischen den materiellen Punkten).

Hierbei ist (b) nur ein wichtiger Specialfall von (c). Eine wirkliche Theorie liegt erst dann vor, wenn die Kraftgesetze gegeben sind; die Kräfte müssen zunächst nur dem Gesetz der Gleichheit von actio und reactio Genüge leisten, damit ein System von Punkten, die durch Kräfte dauernd räumlich aneinander gekettet sind, sich wie *ein* materieller Punkt verhalte. Zusammen mit Newtons Kraftgesetz der Gravitation bilden diese Grundsätze die Basis der klassischen Mechanik der Gestirne. In dieser Mechanik Newtons tritt nun der Raum K_0 in einer Art auf, die gegenüber der obigen, von starren Körpern herkommenden Konzeption des Raumes ein neues Moment enthält: nicht für jedes K_0 beanspruchen (bei gegebenem Kraftgesetz) (b) und (c) Gültigkeit, sondern nur für solche K_0 von geeignetem Bewegungszustande (Inertialsysteme). Dadurch wird dem Koordinatenraum eine selb-

ständige physikalische Eigenschaft beigelegt, die dem rein geometrischen Raumbegriffe fremd ist—ein Umstand, der Newton erhebliches Kopfzerbrechen verursachte (Eimer-Versuch).[2]

Die klassische Mechanik ist nur ein allgemeines Schema; zu einer Theorie wird sie erst dadurch, dass die Kraftgesetze (d) explicite angegeben werden, wie es durch Newton für die Himmelsmechanik mit so ausserordentlichem Erfolg getan wurde. Vom Standpunkte des Zieles möglichster logischer Einfachheit der Grundlagen ist es ein Mangel dieser theoretischen Methode, dass die Kraftgesetze nicht durch logisch-formale Gesichtspunkte gewonnen werden können, sodass ihre Wahl weitgehend a priori willkürlich ist. Auch Newtons Kraftgesetz der Gravitation ist ausschliesslich durch den *Erfolg* gegenüber anderen denkbaren Kraftgesetzen ausgezeichnet.

Trotzdem wir heute sicher wissen, dass die klassische Mechanik als eine die ganze Physik beherrschende Basis versagt, steht sie doch immer noch im Zentrum des physikalischen Denkens. Der Grund hierfür liegt darin, dass wir trotz der bedeutsamen seither erreichten Fortschritte noch nicht zu einer neuen Basis der Physik gelangt sind, von der wir sicher sein dürfen, dass aus ihr die ganze Mannigfaltigkeit der erforschten Erscheinungen und erfolgreichen theoretischen Teilsysteme sich logisch deduzieren liesse. Wie es damit steht, will ich im Folgenden kurz zu skizzieren versuchen.

Zuerst suchen wir uns darüber klar zu werden, inwieweit sich das System der klassischen Mechanik als geeignet erwiesen hat, als Basis für die gesamte Physik zu dienen. Da es uns hier nur auf das Fundament der Physik und dessen Wandlungen ankommt, brauchen wir uns mit den rein *formalen* Fortschritten der Mechanik nicht besonders zu beschäftigen (Lagrangesche Gleichungen, kanonische Gleichungen u. s. w.). Nur *eine* Bemerkung scheint unerlässlich. Der Begriff "materieller Punkt" ist grundlegend für die

[2] Diesem Mangel der Theorie konnte nur durch eine solche Formulierung der Mechanik abgeholfen werden, welche bezüglich aller K_0 Gültigkeit beansprucht. Dies ist einer der Schritte, die zur allgemeinen Relativitäts-Theorie hinführen. Ein zweiter, ebenfalls erst durch die allgemeine Relativitäts-Theorie beseitigter Mangel ist der, dass es innerhalb der Mechanik keinen Grund gibt für die Gleichheit der trägen und der schweren Masse des materiellen Punktes.

Mechanik. Wenn wir nun die Mechanik für ein körperliches Objekt suchen, das selber *nicht* als materieller Punkt behandelt werden kann—was streng genommen für jedes "sinnlich wahrnehmbare" Objekt der Fall ist—so entsteht die Frage: Wie ist das Objekt aus materiellen Punkten aufgebaut zu denken und was für Kräfte haben wir als zwischen ihnen wirkend anzunehmen? Die Stellung dieser Frage ist unerlässlich, wenn die Mechanik darauf Anspruch machen soll, die Objekte *vollständig* zu beschreiben.

Es liegt in der natürlichen Tendenz der Mechanik, diese materiellen Punkte sowie die Gesetze der zwischen ihnen wirkenden Kräfte als unveränderlich anzunehmen, da ja eine zeitliche Aenderung ausserhalb einer theoretischen Deutbarkeit durch die Mechanik läge. Hieraus ersieht man, dass die klassische Mechanik notwendig zu einer atomistischen Konstruktion der Materie hinführt. Man sieht hier besonders deutlich, wie sehr jene Erkenntnistheoretiker irren, welche glauben, dass die Theorie auf induktivem Wege aus der Erfahrung hervorgehe, von welchem Irrtum selbst der überlegene Newton sich nicht freihalten konnte ("Hypotheses non fingo").

Aus der Gefahr, sich diesem Gedankengang (Atomistik) folgend ins Uferlose zu verlieren, rettete sich die Wissenschaft zunächst in folgender Weise. Die Mechanik eines Systems ist bestimmt, wenn seine potentielle Energie in Abhängigkeit von seiner Konfiguration gegeben ist. Sind nun die wirkenden Kräfte so beschaffen, dass sie die Aufrechterhaltung gewisser Ordnungseigenschaften der Konfiguration des Systems garantieren, so kann man die Konfiguration durch eine verhältnismässig geringe Anzahl von Konfigurations-Variabeln q, genügend genau beschreiben; die potentielle Energie berücksichtigt man nur insoweit, als sie von *diesen* Konfigurations-Variabeln abhängig ist (z.B. Beschreibung der Konfiguration eines praktisch starren Körpers durch 6 Variable).

Eine zweite Anwendungsweise der Mechanik, welche es vermeidet, eine Unterteilung der Materie bis zu ihren "wirklichen" materiellen Punkten zu berücksichtigen, ist die Mechanik der sogenannten kontinuierlich verbreiteten Massen. Diese ist durch die Fiktion charakterisiert, dass die Massendichte und Geschwindigkeit der Materie kontinuierlich

von den Koordinaten und der Zeit abhänge und dass der nicht explizit gegebene Teil der Wechselwirkungen sich als Flächenkräfte (Druckkräfte) auffassen lassen, welche ebenfalls kontinuierliche Funktionen des Ortes sind. Hierher gehört die Hydrodynamik und die Theorie der Elastizität fester Körper. Diese Theorien vermeiden die explizite Einführung materieller Punkte durch Fiktionen, denen im Sinne des Fundamentes der klassischen Mechanik nur eine approximative Bedeutung zukommen kann.

Abgesehen von der grossen *praktischen* Bedeutung dieser Disziplinen haben sie durch die Erweiterungen der mathematischen Begriffswelt diejenigen formalen Hilfsmittel geschaffen (partielle Differentialgleichungen), welche für die späteren Versuche einer gegenüber der Newton'schen neuartigen Fundierung der gesamten Physik nötig waren.

Diese beiden Anwendungsarten der Mechanik gehören der sogenannten "phänomenologischen" Physik an. Für sie ist charakteristisch, dass sie sich möglichst erlebnisnaher Begriffe bedient, dafür aber auf Einheitlichkeit der Grundlagen weitgehend verzichtet. Wärme, Elektrizität und Licht werden durch besondere Zustandsvariable und Material-Konstanten neben dem mechanischen Zustande beschrieben, und alle diese Variable in ihren gegenseitigen und zeitlichen Abhängigkeit zu bestimmen war ein in der Hauptsache nur auf empirischem Wege lösbares Problem. Viele Zeitgenossen von Maxwell sahen in einer solchen Darstellungsweise das Endziel der Physik, die sie wegen der relativen Erlebnisnähe der gebrauchten Begriffe für rein induktiv aus den Erlebnissen ableitbar hielten. St. Mill und E. Mach vertraten erkenntnistheoretisch ungefähr diesen Standpunkt.

Es ist nach meiner Ansicht die grösste Leistung der Newton'schen Mechanik, dass ihre konsequente Anwendung zur Ueberwindung dieses (phänomenologischen) Standpunktes führte, und zwar auf dem Gebiete der Wärme-Erscheinungen. Dies geschah durch die kinetische Gas-Theorie und durch die statistische Mechanik überhaupt. Erstere verknüpfte die Zustandsgleichung der idealen Gase, Viskosität, Wärmeleitung und Diffusion der Gase, Radiometer-Erscheinungen der Gase und lieferte die logische Verbindung von Phänomenen, welche vom Erlebnisstandpunkte nicht das Geringste miteinander

zu tun hatten. Letztere lieferte eine mechanische Deutung der thermodynamischen Begriffe und Gesetze sowie die Erkenntnis von der Grenze des Anwendungsbereiches der Begriffe und Gesetze der klassischen Wärmelehre. Diese kinetische Theorie, welche die phänomenologosche Physik bezüglich der logischen Einheitlichkeit der Grundlagen weit übertraf, lieferte ferner für die wahre Grösse der Atome und Moleküle bestimmte Werte, die sich durch mehrere von einander unabhängige Methoden ergaben und so jedem vernünftigen Zweifel entrückt wurden. Diese entscheidenden Fortschritte wurden dadurch erkauft, dass den materiellen Punkten reale Gebilde (Atome bezw. Moleküle) zugeordnet wurden, deren konstruktiv-spekulativer Charakter offenkundig war; niemand konnte hoffen, ein Atom je "unmittelbar wahrzunehmen." Gesetze über beobachtungsnahe Zustandsgrössen (z.B. Temperatur, Druck, Geschwindigkeit) aber wurden aus den Grundbegriffen durch komplizierte Rechnungen abgeleitet. So wurde die ursprünglich mehr "phänomenologisch" aufgebaute Physik (wenigstens ein Teil derselben) unter Zugrundelegung der Newton'schen Mechanik für die Atome bezw. Moleküle auf eine erlebnisfernere aber einheitlichere Basis zurückgeführt.

§ 3. DER FELDBEGRIFF.

Weit weniger erfolgreich als in den bisher genannten Gebieten ist die Newton'sche Mechanik gegenüber den optischen und elektrischen Phänomenen gewesen. Wohl suchte Newton in seiner Korpuskulartheorie des Lichtes dieses auf die Bewegungen von materiellen Punkten zurückzuführen. Als aber die Erscheinungen der Polarisation, Beugung und Interferenz des Lichtes zu immer unnatürlicheren Modifikationen dieser Theorie zwangen, setzte sich Huyghens' Undulationstheorie des Lichtes durch, welche ihre Entstehung wohl hauptsächlich Erscheinungen der Krystalloptik und der bereits einigermassen ausgearbeiteten Theorie des Schalles verdankte. Die Huyghenssche Theorie fusste zwar zunächst ebenfalls auf der klassischen Mechanik. Aber als Träger der Wellenbewegungen musste der alle Körper durchdringende Aether angenommen werden, dessen Aufbau aus materiellen Punkten durch kein bekanntes Phänomen nahegelegt wurde. Ueber

die ihn beherrschenden inneren Kräfte sowie die Kräfte, die zwischen ihm und der "ponderabeln" Materie wirken, konnte man nie ins Klare kommen, sodass das Fundament dieser Theorie immer in Dunkel gehüllt blieb. Die wahre Grundlage war eine partielle Differentialgleichung, deren Zurückführung auf mechanische Elemente stets problematisch blieb.

Zur theoretischen Erfassung der elektrischen und magnetischen Phänomene wurden ebenfalls Massen besonderer Art eingeführt, zwischen welchen Fernkräfte von ähnlicher Art wie die Newton'schen Gravitationskräfte angenommen wurden. Aber diesen besonderen Arten von Materie schien die fundamentale Eigenschaft der Trägheit nicht zuzukommen, und die Kräfte, welche zwischen diesen Massen und der ponderabeln Materie wirkten, blieben in Dunkel gehüllt. Dazu kam der polare Charakter dieser Arten Materie, welcher in das Schema der klassischen Mechanik nicht hineinpasste. Noch unbefriedigender wurde die Basis der Theorie, als elektrodynamische Phänomene bekannt wurden; obwohl diese Phänomene dazu führten, die magnetischen Phänomene auf elektrodynamische zurückzuführen und so die Hypothese der magnetischen Massen entbehrlich zu machen. Dieser Fortschritt muste nämlich durch Komplizierung der zwischen bewegten elektrischen Massen anzunehmenden Wechselwirkungskräfte erkauft werden.

Die Erlösung aus dieser unerquicklichen Sachlage durch Faradays und Maxwells Theorie des elektrischen Feldes bedeutet wohl die tiefgehendste Umwälzung, welche das Fundament der Physik seit Newton erfahren hat. Wieder ist es ein Schritt in der Richtung konstruktiver Spekulation, welcher die Distanz zwischen der Grundlage der Theorie und dem Sinnlich-Erlebbaren vergrösserte. Die Existenz des Feldes manifestiert sich nämlich nur dann, wenn elektrisch geladene Körper in dasselbe hineingebracht werden. Die Maxwell'schen Differentialgleichungen verknüpfen die räumlichen und zeitlichen Differentialquotienten des elektrischen und magnetischen Feldes. Die elektrischen Massen sind nur Stellen nicht verschwindender Divergenz des elektrischen Feldes. Lichtwellen erscheinen als undulatorische elektromagnetische Feldprozesse im Raume.

Allerdings suchte Maxwell seine Feldtheorie noch me-

chanisch zu deuten durch mechanische Aethermodelle. Solche Versuche traten aber seit der von allem unnötigen Beiwerk gereinigten Darstellung von Heinrich Hertz allmählich in den Hintergrund, so dass schliesslich in dieser Theorie das Feld jene fundamentale Rolle übernahm, welche in der Newton'schen Mechanik die Massenpunkte spielten. Dies gilt zunächst allerdings nur für die elektromagnetschen Felder im leeren Raum.

Für das Innere der Materie war die Theorie zunächst noch recht unbefriedigend, indem dort zwei elektrische Vektoren eingeführt werden mussten, welche durch von der Natur des Mediums abhängige Beziehungen verknüpft waren, welche Beziehungen keiner theoretischen Analyse zugänglich waren. Analog war es mit dem magnetischen Felde sowie mit der Beziehung zwischen elektrischer Stromdichte und dem Felde.

Hier schaffte H. A. Lorentz einen Ausweg, der auch zugleich den Weg wies zu einer von Willkür einigermassen freien Elektrodynamik bewegter Körper. Seine Theorie war auf folgenden Grund-Hypothesen aufgebaut.

Sitz des Feldes ist überall (auch im Innern der ponderabeln Körper) der leere Raum. Die Beteiligung der Materie an den elektromagnetischen Vorgängen beruht allein darauf, dass die Elementarpartikeln der Materie unveränderliche elektrische Ladungen tragen und dadurch einerseits ponderomotorischen Wirkungen ausgesetzt sind, andererseits felderzeugend wirken. Die Elementarpartikeln folgen Newtons Bewegungsgesetz für den materiellen Punkt.

Auf solcher Basis erreichte H. A. Lorentz eine Synthese der Newtonschen Mechanik und der Maxwellschen Feldtheorie. Die Schwäche dieser Theorie lag darin, dass sie das Geschehen durch eine Kombination von partiellen Differentialgleichungen (Maxwellsche Feldgleichungen für den leeren Raum) und totalen Differentialgleichungen (Punkt-Bewegungsgleichungen) zu bestimmen suchte, was offenbar unnatürlich war. Das Unbefriedigende dieser Auffassung zeigte sich äusserlich darin, dass es nötig war, die Teilchen als endlich ausgedehnt zu betrachten, damit das an ihrer Oberfläche bestehende elektromagnetische Feld nicht unendlich gross werde. Auch lieferte diese Theorie keinen Aufschluss über die Natur der gewaltigen Kräfte, welche die elektrischen Massen an den einzelnen

Partikeln festhalten. H. A. Lorentz nahm diese von ihm wohlbemerkten Schwächen seiner Theorie zunächst in den Kauf, um die Erscheinungen wenigsten im Grossen Ganzen richtig darzustellen.

Ueber den Rahmen der Lorentzschen Theorie wies ferner folgende Erwägung hinaus. In der Umgebung eines elektrisch geladenen Körpers existiert ein magnetisches Feld, welches einen (scheinbaren) Beitrag zu seiner trägen Masse liefert. Sollte sich nicht die *ganze* Trägheit der Partikeln elektromagnetisch erklären lassen? Dies würde sich offenbar nur dann befriedigend durchführen lassen, wenn sich die Partikeln als reguläre Lösungen der elektromagnetischen partiellen Differentialgleichungen deuten liessen. Die Maxwellschen Gleichungen in ihrer ursprünglichen Gestalt erlaubten aber eine solche Auffassung nicht, weil ihre entsprechenden Lösungen eine Singularität aufweisen. Die Theoretiker suchten das Ziel deshalb lange Zeit hindurch durch eine Modifikation der Maxwell'schen Gleichungen zu erreichen, welche Bestrebung aber nicht von Erfolg gekrönt war. So blieb das Ziel der Aufstellung einer ausschliesslich auf den Feldbegriff gegründeten elektromagnetischen Theorie der Materie zunächst unerfüllt, obwohl gegen dessen Erreichbarkeit prinzipiell nichts eingewendet werden konnte. Was von der weiteren Bemühung in dieser Richtung abschreckte, war das Fehlen einer zu der Lösung führenden systematischen Methode. Eines aber erscheint mir sicher: im Fundamente einer konsequenten Feldtheorie darf neben dem Feldbegriff nicht der Partikelbegriff auftreten; die ganze Theorie muss einzig auf partielle Differentialgleichungen und deren singularitätsfreie Lösungen gegründet sein.

§ 4. DIE RELATIVITÄTSTHEORIE.

Es gibt keine induktive Methode, welche zu den Grundbegriffen der Physik führen könnte. Die Verkennung dieser Tatsache war der philosophische Grundirrtum so mancher Forscher des 19. Jahrhunderts; sie war wohl der Grund dafür, dass sich die Molekulartheorie und die Maxwellsche Theorie erst verhältnismässig spät durchsetzen konnten. Logisches Denken ist notwendig deduktiv, auf hypothetische Begriffe und Axiome gegründet. Wie sollen wir erwarten, letztere so

wählen zu können, dass wir auf die Bewährung ihrer Konsequenzen hoffen dürfen?

Der günstigste Fall liegt offenbar dann vor, wenn die neuen Grundhypothesen durch die Erlebniswelt selbst nahegelegt werden. Die Hypothese von der Nichtexistenz eines perpetuum mobile als Grundlage für die Thermodynamik ist ein solches Beispiel einer durch die Erfahrung nahegelegten Ausgangshypothese; ebenso Galileis Trägheitsprinzip. Von solcher Art sind auch die Grundhypothesen der Relativitätstheorie, welche zu einer ungeahnten Erweiterung und Vertiefung der Feldtheorie und zu einer Ueberwindung der Grundlagen der klassischen Mechanik geführt hat.

Die Erfolge der Maxwell-Lorentzschen Theorie erzeugten grosses Vertrauen in die Gültigkeit der elektromagnetischen Gleichungen des Vakuums, im Besonderen also auch in die Aussage, dass das Licht sich "im Raume" mit einer bestimmten konstanten Geschwindigkeit c ausbreitet. Gilt diese Aussage von der Konstanz der Licht-Ausbreitungs-Geschwindigkeit inbezug auf beliebige Inertialsysteme? Wenn dies nicht der Fall wäre, so wäre ein bestimmtes Inertialsystem oder genauer ein bestimmter Bewegungs-Zustand (eines Bezugskörpers) von allen anderen ausgezeichnet. Dagegen sprachen aber alle mechanischen und elektromagnetisch-optischen Erfahrungstatsachen.

Es war also geboten, die Gültigkeit des Gesetzes der Konstanz der Lichtausbreitung für alle Inertialsysteme zum Prinzip zu erheben. Daraus folgte, dass die räumlichen Koordinaten X_1, X_2, X_3 und die Zeit X_4 beim Uebergang von einem Inertialsystem zu einem andern sich gemäss der "Lorentz-Transformation" transformieren müssen, welche durch die Invarianz des Ausdruckes

$$ds^2 = dx_1^2 + dx_2^2 + dx_3^2 - dx_4^2$$

charakterisiert ist (wenn man die Zeiteinheit so wählt, dass die Lichtgeschwindigkeit c gleich 1 wird).

Dadurch verlor die Zeit ihren absoluten Charakter und wurde den "räumlichen" Koordinaten als algebraisch (nahezu) gleichartige Bestimmungsgrösse zugeordnet; der absolute Charakter der Zeit und im Besonderen der Gleichzeitigkeit war zerstört und die vierdimensionale Beschreibung als einzig adaequate eingeführt.

Damit darüber hinaus der Aequivalenz aller Inertialsysteme inbezug auf alles Naturgeschehen Rechnung getragen sei, muss die Invarianz aller physikalischen Gleichungssysteme, welche allgemeine Gesetze ausdrücken, gegenüber Lorentz-Transformationen gefordert werden. Die Ausführung dieser Forderung bildet den Inhalt der speziellen Relativitätstheorie.

Diese Theorie passt zwar zu den Maxwellschen Gleichungen, ist aber unvereinbar mit der Grundlage der klassischen Mechanik. Zwar lassen sich die Bewegungsgleichungen des materiellen Punktes so modifizieren (und mit ihnen die Ausdrücke für Impuls und kinetische Energie des Massenpunktes), dass der Theorie Genüge geleistet wird. Aber der Begriff der Wechselwirkungskraft (und damit der Begriff der potentiellen Energie eines Systems) verliert seine Grundlage, weil er auf demjenigen der absoluten Gleichzeitigkeit beruht. An die Stelle der Kraft tritt das durch Differentialgleichungen beherrschte Feld.

Da diese Theorie nur Wechselwirkungen durch Felder zulässt, verlangt sie eine Feldtheorie der Gravitation. Es ist auch in der Tat nicht schwer, eine solche aufzustellen, in welcher wie in Newtons Theorie das Gravitationsfeld sich auf einen Skalar zurückführen lässt, der einer partiellen Differentialgleichung genügt. Aber die in Newtons Gravitationstheorie ausgedrückten Erfahrungstatsachen führen auf einen andern Weg, den der allgemeinen Relativitätstheorie.

In der klassischen Mechanik ist es ein unbefriedigender Punkt, dass *dieselbe* Massenkonstante in zwei verschiedenen Weisen im Fundament auftritt; als "träge" Masse im Bewegungsgesetz und als "schwere" Masse im Gravitationsgesetz. Infolgedessen ist die Beschleunigung eines Körpers im reinen Schwerfeld unabhängig vom Material oder: im *gleichförmig beschleunigten* Koordinatensystem (beschleunigt gegenüber einem "Inertialsystem") vollziehen sich die Bewegungen wie in einem homogenen Gravitationsfelde (gegenüber einem "ruhenden" Koordinatensystem). Nimmt man an, dass die Aequivalenz dieser beiden Fälle eine vollständige sei, so erreicht man eine Anpassung des theoretischen Denkens an die Tatsache der Gleichheit der trägen und schweren Masse.

Damit fällt aber auch die prinzipielle Bevorzugung der "Inertialsysteme" weg und es werden auch *nichtlineare*

Transformationen der Koordinaten (X_1, X_2, X_3, X_4) als gleichberechtigt zugelassen. Nimmt man eine solche Transformation von einem Koordinatensystem der speziellen Relativitätstheorie aus vor, so geht deren Metrik

$$ds^2 = dx_1^2 + dx_2^2 + dx_3^2 - dx_4^2$$

über in eine allgemeine (Riemann'sche) Metrik vom Baue

$$ds^2 = g_{\mu\nu} dx_\mu dx_\nu,$$

(über μ und ν summiert),

wobei die inbezug auf μ und ν symetrischen $g_{\mu\nu}$ gewisse Funktionen von $x_1 \cdots x_4$ sind, welche sowohl die metrischen Eigenschaften als auch das Gravitationsfeld des Raumes gegenüber dem neuen Koordinatensystem beschreiben.

Diesen Fortschritt in der Interpretation des mechanischen Fundamentes hat man aber damit zu erkaufen, dass—wie eine nähere Betrachtung lehrt—die neuen Koordinaten nicht mehr so unmittelbar als Messergebnisse an starren Körpern und Uhren gedeutet werden können wie im ursprünglichen System (einem Inertialsystem mit verschwindendem Gravitationsfelde).

Den Uebergang zur allgemeinen Relativitätstheorie schafft nun die Annahme, dass eine solche Darstellung der genannten Feldeigenschaften des Raumes durch Funktionen $g_{\mu\nu}$ (bezw. durch eine Riemann-Metrik) auch in dem *allgemeinen* Falle berechtigt sei, in welchem es kein Koordinatensystem gibt, inbezug auf welches die Metrik die einfache quasi-euklidische Form der speziellen Relativitätstheorie annimmt.

Die Koordinaten drücken nun an sich nicht mehr metrische Zusammenhänge aus, sondern nur noch das "Benachbartsein" von beschriebenen Dingen, deren Koordinaten wenig voneinander abweichen. Alle singularitätsfreien Transformationen der Koordinaten sind zulässig. Nur solche Gleichungen sind als Ausdruck allgemeiner Naturgesetze sinnvoll, welche bezüglich in diesem Sinne beliebiger Transformationen kovariant sind. (Postulat der allgemeinen Kovarianz).

Erstes Ziel der allgemeinen Relativitätstheorie war eine vorläufige Fassung, welche unter Verzicht auf gewisse Forderungen innerer Geschlossenheit möglichst einfach mit dem "direkt Erlebbaren" verbunden war. Bei Beschränkung auf

die reine Gravitations-Mechanik lieferte die Newtonsche Gravitationstheorie das Vorbild. Diese vorläufige Fassung lässt sich so charakterisieren:

(1) Der Begriff des materiellen Punktes und seiner Masse wird beibehalten. Für ihn wird ein Bewegungsgesetz angegeben, welches die Uebersetzung des Trägheitsgesetzes in die Sprache der allgemeinen Relativitätstheorie ist. Es ist dies ein System von totalen Differentialgleichungen, das der geodätischen Linie.

(2) An die Stelle des Newtonschen Wechselwirkungs-Gesetzes durch Gravitation tritt das System der einfachsten allgemein kovarianten Differentialgleichungen, welches für den $g_{\mu\nu}$-Tensor aufgestellt werden kann. Es entsteht durch Nullsetzung des einmal verjüngten Riemannschen Krümmungs-Tensors ($R_{\mu\nu} = 0$).

Diese Formulierung erlaubt die Behandlung des Planetenproblems, genauer die Behandlung des Problems der Bewegung von materiellen Punkten von praktisch verschwindender Masse in einem von einem als "ruhend" gedachten Massenpunkte erzeugten (zentralsymetrischen) Gravitationsfelde. Sie trägt weder der Rückwirkung der "bewegten" Massenpunkte auf das Gravitationsfeld Rechnung, noch stellt sie dar, wie die zentrale Masse das Gravitationsfeld erzeugt.

Die Analogie zur klassischen Mechanik zeigt folgenden Weg zur Vervollständigung der Theorie. Man setzt als Feldgleichungen

$$R_{ik} - \tfrac{1}{2} g_{ik} R = - T_{ik},$$

wobei R den Skalar der Riemannkrümmung, T_{ik} den Energietensor der Materie in einer phänomenologische Darstellung bedeutet. Die linke Seite ist so gewählt, dass ihre Divergenz identisch verschwindet. Das hieraus folgende Verschwinden der Divergenz der rechten Seite liefert die "Bewegungsgleichungen" der Materie in Form von partiellen Differentialgleichungen für den Fall, dass T_{ik} für die Beschreibung der Materie nur *vier* voneinander unabhängige weitere Funktionen einführt (z.B. Dichte, Druck und Geschwindigkeitskomponenten, wobei zwischen letzteren eine Identität, zwischen Druck und Dichte eine Bedingungsgleichung besteht).

Bei dieser Formulierung ist die ganze Gravitations-

mechanik auf die Lösung eines einzigen Systems von kovarianten partiellen Differentialgleichungen reduziert. Diese Theorie vermeidet alle inneren Mängel, welche wir dem Fundament der klassischen Mechanik zur Last gelegt haben; sie genügt—soweit wir wissen—zur Darstellung der Erfahrungen der Himmelsmechanik. Sie gleicht aber einem Gebäude, dessen einer Flügel aus vorzüglichem Marmor (linke Seite der Gleichung), dessen anderer Flügel aus minderwertigem Holze gebaut ist (rechte Seite der Gleichung). Die phänomenologische Darstellung der Materie ist nämlich nur ein roher Ersatz für eine Darstellung, welche allen bekannten Eigenschaften der Materie gerecht würde.

Es macht keine Schwierigkeit, die Maxwellsche Theorie des elektromagnetischen Feldes mit der Theorie des Gravitationsfeldes zu verknüpfen, wenn man sich auf den Raum ohne ponderable Materie und ohne elektrische Dichte beschränkt. Man hat einfach auf die rechte Seite obiger Gleichung für T_{ik} den Energietensor des elektromagnetischen Vakuumfeldes zu setzen und dem so modifizierten Gleichungssystem die allgemein kovariant geschriebenen Maxwell'schen Feldgleichungen des Vakuums zuzuordnen. Es bestehen dann zwischen all diesen Gleichungen hinreichend viele Differential-Identitäten, um deren Kompatibilität zu gewährleisten. Es sei hinzugefügt, dass diese notwendige formale Eigenschaft des gesamten Gleichungssystems die Wahl des Vorzeichens des Gliedes T_{ik} offen lässt, was sich später als bedeutsam erwiesen hat.

Das Streben nach möglichster Einheitlichkeit der Grundlagen der Theorie hat verschiedene Versuche veranlasst, das Gravitationsfeld und das elektromagnetische Feld unter einen einheitlichen formalen Gesichtspunkt zu bringen. Hier ist besonders die fünfdimensionale Theorie von Kaluza und Klein zu nennen. Nach sorgfältiger Abwägung dieser Möglichkeit halte ich es aber doch für richtiger, den genannten Mangel an innerer Einheitlichkeit der ursprünglichen Theorie hinzunehmen, weil mir der Inbegriff der der fünfdimensionalen Theorie zugrundeliegenden Hypothesen nicht weniger Willkür zu enthalten scheint als die ursprüngliche Theorie. Dies gilt auch von der projektiven Abart der Theorie, welche insbesondere von v. Dantzig und von Pauli sorgfältig ausgearbeitet worden ist.

Die letzten Ausführungen bezogen sich ausschliesslich auf die Theorie des materie-freien Feldes. Wie soll man aber von hier aus zu einer vollständigen Theorie der atomistisch konstituierten Materie gelangen? Jedenfalls müssen in einer solchen Theorie Singularitäten ausgeschlossen werden, da sonst die Differentialgleichungen das Gesamtfeld nicht vollständig bestimmen. Es besteht hier für die Feldtheorie der allgemeinen Relativität dasselbe Problem einer feldtheoretischen Darstellung der Materie wie ursprünglich für die Maxwell'sche Theorie allein.

Auch hier führt der Versuch einer feldtheoretischen Konstruktion der Teilchen scheinbar auf Singularitäten. Auch hier suchte man diesem Uebelstande durch Einführung neuer Feldvariabeln und Komplizierung und Erweiterung des Systems der Feldgleichungen abzuhelfen. Vor kurzem entdeckte ich aber zusammen mit Herrn Rosen, dass die oben angedeutete einfachste Kombination der Feldgleichungen von Gravitation und Elektrizität zentralsymmetrische Lösungen liefert, welche singularitätsfrei dargestellt werden können (die bekannten zentralsymmetrischen Lösungen von Schwarzschild für das reine Gravitationsfeld und die von Reissner für das elektrische Feld unter Berücksichtigung seiner Gravitationswirkung). Hierüber wird im übernächsten Paragraphen kurz berichtet. Es scheint hier eine von zusätzlichen Hypothesen freie reine Feldtheorie der Materie und ihrer Wechselwirkungen möglich, deren Prüfung an der Erfahrung keine anderen als allerdings bedeutende rein mathematische Schwierigkeiten entgegenstehen.

§ 5. QUANTENTHEORIE UND FUNDAMENT DER PHYSIK.

Die theoretischen Physiker der gegenwärtigen Generation erwarten sich die Aufstellung eines neuen theoretischen Fundamentes der Physik unter Verwendung von fundamentalen Begriffen, die von denjenigen der bisher betrachteten Feldtheorie erheblich abweichen. Der Grund hiefür liegt darin, dass man sich genötigt gesehen hat, für die mathematische Darstellung der sogenannten Quantenphänomene neuartige Betrachtungsweisen zu verwenden.

Während nämlich das durch die Relativitätstheorie aufgedeckte Versagen der klassischen Mechanik mit der Endlich-

keit (dem nicht ∞-sein) der Lichtgeschwindigkeit zusammenhängt, entdeckte man am Anfange unseres Jahrhunderts andersartige Inkongruenzen zwischen Folgerungen der Mechanik und den experimentellen Tatsachen, welche mit der Endlichkeit (dem nicht 0-sein) der Planck'schen Konstante h zusammenhängen. Während nämlich die Molekular-Mechanik ein im wesentlichen *proportionales* Absinken der (monochromatischen) Strahlungsdichte und des Wärmeinhaltes der festen Körper mit sinkender absoluter Temperatur fordert, zeigte die Erfahrung ein viel rascheres Absinken dieser Energiegrössen mit der Temperatur. Um dieses Verhalten theoretisch zu deuten, musste man annehmen, dass die Energie mechanischer Systeme nicht beliebige Werte annehmen kann sondern nur gewisse diskrete Werte, deren mathematische Ausdrücke stets von der Planckschen Konstante h abhängig waren. Auch für die Theorie des Atoms (Bohrsche Theorie) war diese Auffassung wesentlich. Für den Uebergang dieser Zustände ineinander—mit oder ohne Emission bezw. Absorption von Strahlung—konnte man nicht kausale sondern nur statistische Gesetze angeben, ebenso für den ungefähr zu der gleichen Zeit genauer erforschten radioaktiven Zerfall von Atomen. Mehr als zwei Jahrzehnte lang bemühten sich die Physiker vergebens, eine einheitliche Deutung jenes quantenhaften Charakters der Systeme und Vorgänge zu finden. Eine solche gelang aber vor etwa einem Jahrzehnt durch zwei scheinbar gänzlich verschiedene theoretische Methoden, von denen die eine auf Heisenberg und Dirac, die andere auf de Broglie und Schrödinger zurückgeht; die mathematische Gleichwertigkeit beider Theorien wurde dann bald von Schrödinger erkannt. Ich will hier den der Denkweise des Physikers näher liegenden Gedanken-Weg von de Broglie-Schrödinger zu skizzieren versuchen, um dann allgemeine Ueberlegungen daran zu knüpfen.

Die Frage ist zunächst: Wie kann man einem im Sinne der klassischen Mechanik gegebenen System (die Energie-Funktion ist eine gegebene Funktion der Koordinaten q_r und der zugehörigen Momente p_r) eine diskrete Folge von Energiewerten H_σ zuordnen? Die Plancksche Konstante h ordnet den Energiewerten H_σ die Frequenzwerte $1/h\ H_\sigma$ zu. Es genügt also, dem System eine diskrete Folge von *Frequenz-*

werten zuzuordnen. Dies erinnert daran, dass in der Akustik der Fall auftritt, dass einer partiellen linearen Differentialgleichung (bei gegebenen Grenzbedingungen) eine Reihe diskreter Frequenzwerte zugeordnet ist, nämlich die der sinusartig periodischen Lösungen. Schrödinger stellte sich dementsprechend die Aufgabe, der gegebenen Energiefunktion $\mathcal{E}(q_r, p_r)$ eine partielle Differentialgleichung für eine skalare Funktion ψ zuzuordnen, wobei die q_r und die Zeit t die unabhängigen Variabeln sind. Dies gelang ihm (für eine komplexe Funktion ψ) in solcher Weise, dass die von der statistischen Theorie geforderten theoretischen Werte der Energie (H_σ) wirklich in befriedigender Weise aus den periodischen Lösungen der Gleichung sich ergaben.

Allerdings erwies es sich nicht als möglich, einer bestimmten Lösung $\psi(q_r, t)$ der Schrödinger-Gleichung einen bestimmten Bewegungsvorgang im Sinne der Mechanik materieller Punkte zuzuordnen; d.h. der ψ-Funktion entspricht nicht oder wenigstens nie *genau* eine Darstellung der q_r als Funktion der Zeit t. Eine Interpretation der physikalischen Bedeutung der ψ-Funktion erwies sich aber nach Born als in folgender Weise durchführbar: $\psi\bar{\psi}$ (das Quadrat des absoluten Betrages der komplexen Funktion ψ) ist die Wahrscheinlichkeitsdichte für die betrachtete Stelle im Konfigurationsraum der q_r und für den Zeitwert t. Anschaulich, aber etwas ungenau kann man demnach den Inhalt der Schrödinger-Gleichung so charakterisieren: sie bestimmt, wie sich die Wahrscheinlichkeitsdichte einer statistischen System-Gesamtheit im Konfigurationsraum mit der Zeit ändert. Kurz: die Schrödinger-Gleichung bestimmt die Aenderung der Funktion ψ der q_r mit der Zeit.

Es muss erwähnt werden, dass die Ergebnisse der Theorie diejenigen der Punktmechanik als Grenzfälle enthalten, wenn die bei der Lösung des Schrödinger-Problems auftretenden Wellenlängen überall so klein sind, dass die potentielle Energie sich beim Vorschreiten um eine Wellenlänge im Konfigurationsraum nur praktisch unendlich wenig ändert. Dann kann man nämlich folgendes dartun: Man wählt ein Gebiet G_0 im Konfigurationsraum, das zwar (in jeder Richtung) gross gegen die Wellenlänge, aber klein gegen die massgebenden Ausmessungen des Konfigurationsraumes ist. Man kann dann eine

ψ-Funktion für eine Anfangszeit t_0 so wählen, dass sie ausserhalb G_0 verschwindet und sich gemäss der Schrödinger-Gleichung so verhält, dass sie auch für die späteren Zeiten diese Eigenschaft angenähert behält, nur dass das Gebiet G_0 zur Zeit t in ein anderes Gebiet G übergegangen ist. Man kann so angenähert von der "Bewegung" des Gebietes G als ganzen reden und sie durch die Bewegung eines Punktes im Konfigurationsraum approximieren. Diese Bewegung fällt dann mit derjenigen zusammen, welche durch die Gleichungen der klassischen Mechanik gefordert wird.

Interferenzversuche an Teilchenstrahlen lieferten den glänzenden Beweis dafür, dass der von der Theorie angenommene Wellencharakter der Bewegungsvorgänge der Wirklichkeit entspricht. Ferner gelang es der Theorie mit Leichtigkeit, die statistischen Gesetze des Ueberganges eines Systems von einem Quantenzustande zu einem andern unter der Wirkung äusserer Kräfte darzustellen, was vom Gesichtspunkte der klasssischen Mechanik wie ein Wunder erscheint. Die äusseren Kräfte wurden hierbei durch kleine zeitabhängige Zusätze zur potentiellen Energie dargestellt. Während solche Zusätze nach der klassischen Mechanik nur entsprechend kleine Aenderungen des Systems erzeugen können, erzeugen sie nach der Quantenmechanik beliebig grosse Aenderungen, aber mir entsprechend kleiner Wahrscheinlichkeit—durchaus im Einklang mit der Erfahrung. Selbst ein Verständnis für die Gesetze des radioaktiven Zerfalles liefert diese Theorie zumindest in grossen Zügen.

Es dürfte überhaupt wohl kaum jemals eine Theorie aufgestellt worden sein, welche einen Schlüssel zur Deutung und Berechnung so verschiedenartiger Erfahrungsthatsachen geliefert hat wie die Quantenmechanik. Trotzdem glaube ich, dass sie dazu angetan ist, uns beim Suchen nach einem einheitlichen Fundament der Physik in die Irre zu führen; sie ist nämlich nach meiner Ansicht eine *unvollständige* Darstellung der wirklichen Gebilde, wenn auch die einzig zutreffende, welche sich auf die Grundbegriffe materieller Punkt und Kraft bauen lässt (Quantenkorrektur der klassischen Mechanik). Der Unvollständigkeit der Darstellung entspricht aber notwendig der statistische Charakter (Unvollständigkeit) der Gesetzlichkeit. Diese Meinung will ich nun begründen:

Ich frage zunächst: inwiefern beschreibt die ψ-Funktion einen wirklichen Zustand eines mechanischen Systems? Es seien ψ_r die (nach aufsteigenden Werten der Energie geordneten) periodischen Lösungen der Schrödinger-Gleichung. Die Frage, inwieweit die einzelnen ψ_r *vollständige* Beschreibungen von physikalischen Zuständen sind, lasse ich vorläufig offen. Ein System befindet sich zunächst im Zustande ψ_1 niedrigster Energie ε_1. Dann wirke auf das System während einer beschränkten Zeit eine kleine störende Kraft. Für eine spätere Zeit erhält man dann aus der Schrödinger-Gleichung eine ψ-Funktion von der Gestalt

$$\psi = \Sigma c_r \psi_r$$

wobei die c_r (komplexe) Konstante sind. Sind die ψ_r "normiert" so ist $|c_1|$ nahezu gleich 1, $|c_2|$ etc. klein gegen 1. Man kann nun fragen: Beschreibt ψ einen wirklichen Zustand des Systems? Wenn ja, so dürften wir kaum anders können, als diesem Zustand eine bestimmte Energie ε zuzuschreiben [3] und zwar eine solche, die ε_1 um ein geringes übersteigt (Jedenfalls $\varepsilon_1 < \varepsilon < \varepsilon_2$). Eine solche Annahme ist aber mit den Elektronenstoss-Versuchen, wie sie zuerst von J. Franck und G. Hertz durchgeführt worden sind, im Widerspruch, wenn man noch Millikans Nachweis von der diskreten Natur der Elektrizität zuhilfe nimmt. Aus diesen Versuchen folgt nämlich, dass es Energie-Zustände eines Systems, die zwischen den Quanten-Werten liegen, nicht gibt. Hieraus folgt, dass unsere Funktion ψ überhaupt keinen einheitlichen Zustand des Gebildes beschreibt, sondern eine statistische Aussage darstellt, in welcher die c_r Wahrscheinlichkeiten der einzelnen Energie-Werte bedeuten. Es scheint mir deshalb klar, dass die Born'sche statistische Deutung der Aussagen der Quantentheorie die einzig mögliche ist: Die ψ-Funktion beschreibt überhaupt nicht einen Zustand, der einem einzelnen System zukommen könnte; sie bezieht sich vielmehr auf so viele Systeme, eine "System-Gesamtheit" im Sinne der statistischen Mechanik. Wenn die ψ-Funktion abgesehen von besonderen Fällen nur *statistische* Aussagen über messbare

[3] Denn die Energie eines (ruhenden) Gesamtsystems ist gleich dessen Trägheit (als Ganzes) gemäss einem gesicherten Ergebnis der Relativitätstheorie. Diese aber muss einen wohldefinierten, bestimmten Wert haben.

Grössen liefert, so liegt dies also nicht nur daran, dass *der Vorgang der Messung* unbekannte, nur statistisch erfassbare Elemente einführt, sondern eben daran, dass die ψ-Funktion überhaupt nicht den Zustand *eines* Einzelsystems beschreibt. Die Schrödinger-Gleichung bestimmt die zeitlichen Aenderungen, welche die System-Gesamtheit erfährt, sei es ohne, sei es mit äusseren Einwirkungen auf das Einzel-System.

Eine solche Interpretation beseitigt auch eine von mir zusammen mit zwei Mitarbeitern jüngst dargestellte Paradoxie, die sich auf folgenden Fall bezieht:

Ein mechanisches System bestehe aus zwei Teilsystemen A und B, die nur während einer beschränkten Zeit in Wechselwirkung miteinander treten. Die ψ-Funktion vor der Wechselwirkung sei gegeben. Dann liefert die Schrödinger-Gleichung die ψ-Funktion nach der Wechselwirkung. Es werde nun der physikalische Zustand des Teilsystems A durch eine möglichst vollständige Messung bestimmt. Die Quantenmechanik erlaubt dann, aus dem Messungsresultat und der ψ-Funktion des Gesamtsystems die ψ-Funktion des Teilsystems B zu bestimmen. Diese Bestimmung liefert aber ein Ergebnis, das davon abhängt, *welche* Zustandsgrössen von A gemessen werden (z.B. Koordinaten *oder* Momente). Da es nur *einen* physikalischen Zustand von B nach der Wechselwirkung geben kann, welcher vernünftigerweise nicht davon abhängig gedacht werden kann, was für Messungen ich an dem von B getrennten System A vornehme, zeigt dies dass die ψ-Funktion dem physikalischen Zustande *nicht* eindeutig zugeordnet ist. Diese Zuordnung mehrerer ψ-Funktionen zu demselben physikalischen Zustande des Systems B zeigt wieder, dass die ψ-Funktion nicht als (vollständige) Beschreibung eines physikalischen Zustandes (eines Eingelsystems) gedeutet werden kann. Die Zuordnung der ψ-Funktion zu einer System-Gesamtheit beseitigt auch hier jede Schwierigkeit.[4]

Dass die Quantenmechanik in so einfacher Weise Aussagen über (scheinbar) diskontinuierliche Uebergänge von einem

[4] Vornahme einer Messung von A bedeutet nämlich dann Uebergang zu einer engeren System-Gesamtheit. Letztere, (also auch deren ψ-Funktion) hängt davon ab, nach welchem Gesichtspunkte diese Verengung der System-Gesamtheit vorgenommen wird.

Gesamtzustande in einen andern abzuleiten gestattet, ohne wirklich eine Darstellung des eigentlichen Prozesses zu geben, hängt damit zusammen, dass die Theorie in Wahrheit nicht mit dem Einzelsystem sondern mit einer System-Gesamtheit operiert. Die Koeffizienten c_r unseres ersten Beispieles ändern sich eben unter der Wirkung der äusseren Kräfte nur wenig. Man begreift bei dieser Interpretation der Quantenmechanik, wieso diese der Tatsache mit Leichtigkeit gerecht werden kann, dass schwache, störende Kräfte beliebig grosse Aenderungen des physikalischen Zustandes eines Systems bewirken können. Solche störende Kräfte erzeugen nämlich nur entsprechend schwache Aenderungen der *statistischen Dichten* in der System-Gesamtheit, also auch nur unendlich schwache Aenderungen der ψ-Funktion, deren mathematische Beschreibung weit weniger Schwierigkeiten darbietet als die mathematische Darstellung der endlichen Aenderungen, welche ein Teil der Einzelsysteme erleidet. Der am Einzelsysteme sich abspielende Vorgang bleibt freilich bei solcher Betrachtungsweise völlig unaufgeklärt; letzterer ist eben durch die statistische Betrachtungsweise aus der Darstellung völlig eliminiert.

Ich frage nun aber:

Glaubt wirklich irgend ein Physiker, dass wir in diese bedeutenden Veränderungen der Einzelsysteme, ihre Struktur und ihre Kausalzusammenhänge niemals werden einen Einblick erlangen können, trotzdem jene Einzelvorgänge dank der wunderbaren Erfindungen der Wilson-Kammer und des Geiger-Zählers in solche Erlebnisnähe gerückt sind? Dies zu glauben, ist zwar logisch widerspruchsfrei möglich, widerstrebt aber meinem wissenschaftlichen Instinkt so lebhaft, dass ich es nicht unterlassen kann, nach einer vollständigeren Auffassungsweise zu suchen.

Zu diesen Erwägungen kommen noch solche anderer Art hinzu, welche ebenfalls dagegen sprechen, dass die durch die Quantenmechanik eingeschlagene Methode geeignet sei, ein brauchbares Fundament für die ganze Physik zu liefern. In der Schrödinger-Gleichung spielt die absolute Zeit, bezw. die potentielle Energie eine entscheidende Rolle, welche Begriffe durch die Relativitätstheorie als im Prinzip unzulässig erkannt sind. Will man davon loskommen, so muss man statt der

Wechselwirkungskräfte Felder und Feldgesetze zugrunde legen. Dies führt dahin, auf Felder, d.h. Systeme von unendlich vielen Freiheitsgraden die statistische Methode der Quantenmechanik zu übertragen. Obwohl die bisherigen Versuche sich auf lineare Gleichungen beschränken, was doch nach den Ergebnissen der allgemeinen Relativitätstheorie nicht genügen kann, sind die bei den bisherigen überaus scharfsinnigen Bemühungen auftretenden Komplikationen bereits abschreckend gross. Sie müssen sich türmen, wenn man der Forderung der allgemeinen Relativität gerecht werden will, an deren grundsätzlicher Berechtigung doch niemand zweifelt.

Es ist allerdings darauf hingewiesen worden, dass bereits die Einführung eines raum-zeitlichen Kontinuums angesichts der molekularen Struktur allen Geschehens im Kleinen möglicherweise als naturwidrig anzusehen sei. Vielleicht weise der Erfolg von Heisenbergs Methode auf eine rein algebraische Methode der Naturbeschreibung, auf die Ausschaltung kontinuierlicher Funktionen aus der Physik hin. Dann aber muss auch auf die Verwendung des Raum-Zeit-Kontinuums prinzipiell verzichtet werden. Es ist nicht undenkbar, dass der menschliche Scharfsinn einst Methoden finden wird, welche die Beschreitung dieses Weges möglich machen. Einstweilen aber erscheint dieses Projekt ähnlich wie der Versuch, in einem luftleeren Raum zu atmen.

Es ist kein Zweifel, dass die Quantenmechanik ein schönes Stück Wahrheit erfasst hat, und dass es ein Prüfstein eines künftigen theoretischen Fundamentes sein wird, dass sie als Grenzfall aus diesem Fundament deduzierbar sein muss—etwa wie die Elektrostatik aus den Maxwellschen Gleichungen des elektromagnetischen Feldes oder die Thermodynamik aus der klassischen Mechanik. Aber ich glaube, dass beim Aufsuchen jenes Fundamentes die Quantenmechanik nicht als *Ausgangspunkt* dienen kann, so wenig als man umgekehrt ausgehend von der Thermodynamik bezw. statistischen Mechanik auf das Fundament der Mechanik kommen könnte.

Angesichts dieser Sachlage erscheint es mir durchaus gerechtfertigt, die Frage ernsthaft zu erwägen, ob nicht *doch* die Grundlage der Feldphysik mit den Quanten-Tatsachen vereinbar ist. Ist doch diese Grundlage die einzige, welche bei dem

heutigen Stande unserer mathematischen Ausdrucksmittel dem Postulat der allgemeinen Relativität sich anpassen lässt. Die unter den Physikern der Gegenwart vorherrschende Ueberzeugung von der Aussichtslosigkeit eines solchen Versuches dürfte mit der unbegründeten Ueberzeugung zusammenhängen, dass eine solche Theorie in erster Näherung auf die Gleichungen der klassischen Mechanik für die Bewegung der Korpuskeln führen müsse, oder wenigstens auf totale Differentialgleichungen. In Wahrheit ist es aber bisher überhaupt nicht gelungen, Korpuskeln feldtheoretisch singularitätsfrei darzustellen, und wir können über das Verhalten solcher Gebilde a priori gar nichts aussagen. *Eines* aber ist sicher: liefert eine Feldtheorie eine singularitätsfreie Darstellung der Korpuskeln, so ist deren zeitliches Verhalten durch die Differentialgleichungen des Feldes allein bestimmt.

§ 6. RELATIVITÄTSTHEORIE UND KORPUSKELN.

Ich will nun noch zeigen, dass es gemäss der allgemeinen Relativitätstheorie singularitätsfreie Lösungen der Feldgleichungen gibt, welche als Darstellung von Korpuskeln gedeutet werden können. Ich beschränke mich hier auf neutrale Teilchen, weil ich den Gegenstand vor kurzem zusammen mit Herrn Rosen an anderer Stelle ausführlich dargelegt habe, und weil das Wesentliche der Sache sich an diesem Falle völlig darstellen lässt.

Das Gravitationsfeld wird durch den Tensor $g_{\mu\nu}$ vollständig beschrieben. In den Drei-Indices-Symbolen $\Gamma_{\mu\nu}^{\sigma}$ treten auch die kontravarianten $g^{\mu\nu}$ auf, welche als die durch die Determinante $g(=|g_{\alpha\beta}|)$ dividierten Unterdeterminanten der $g_{\mu\nu}$ definiert sind. Damit die R_{ik} bildbar und endlich seien, genügt es nicht, dass es für die Umgebung jedes Punktes des Kontinuums ein Koordinatensystem gebe, in welchem die $g_{\mu\nu}$ und ihre ersten Differentialquotienten stetig und differenzierbar sind, sondern es darf auch die Determinante g nirgends verschwinden. Diese letztere Einschränkung fällt aber weg, wenn man die Differentialgleichungen $R_{ik} = 0$ durch $g^2 R_{ik} = 0$ ersetzt, deren linke Seiten *ganze* rationale Funktionen der g_{ik} und ihrer Ableitungen sind.

Diese Gleichungen haben die von Schwarzschild angegebene zentralsymmetrische Lösung

$$ds^2 = -\frac{1}{1-2m/r}dr^2 - r^2(d\theta^2 + \sin^2\theta d\varphi^2) + \left(1 - \frac{2m}{r}\right)dt^2.$$

Diese Lösung hat bei $r = 2m$ eine Singularität, da der Faktor von $dr^2(g_{11})$ an dieser Hyperfläche ∞ wird. Ersetzt man jedoch die Variable r durch ρ gemäss der Substitution

$$\rho^2 = r - 2m$$

so erhält man

$$ds^2 = -4(2m + \rho^2)d\rho^2 - (2m + \rho^2)^2(d\theta^2 + \sin^2\theta d\varphi^2) + \frac{\rho^2}{2m + \rho^2}dt^2.$$

Diese Lösung verhält sich für alle ρ regulär. Das Verschwinden des Faktors von $dt^2(g_{44})$ für $\rho = 0$ bewirkt zwar, dass hier die Determinante g verschwindet, was aber bei der gewählten Schreibweise der Feldgleichungen keine Singularität bedeutet.

Läuft ρ zwischen $-\infty$ und $+\infty$, so läuft r von $+\infty$ bis $r = 2m$ und dann wieder zurück bis $+\infty$ während solchen r für welche $r < 2m$, überhaupt keine reellen ρ entsprechen. Die Schwarzschildsche Lösung wird also dadurch zu einer regulären Lösung, dass man den physikalischen Raum als aus zwei identischen "Schalen" bestehend darstellt, welche in der Hyperfläche $\rho = 0$ bezw. $r = 2m$ aneinandergrenzen, für welch letztere die Determinante g verschwindet. Eine derartige Verbindung zwischen den beiden (identischen) Schalen wollen wir eine "Brücke" nennen. Die Existenz einer "Brücke" zwischen beiden Schalen im Endlichen entspricht also einem materiellen neutralen Teilchen, welches singularitätsfrei beschrieben ist.

Die Lösung des Problems der Bewegung von neutralen Teilchen kommt offenbar hinaus auf die Auffindung von solchen Lösungen der (nennerfrei geschriebenen) Gravitationsgleichungen, welche mehrere Brücken aufweisen.

Die im Vorigen skizzierte Auffassung wird von vorneherein der atomistischen Struktur der Materie insofern gerecht, als die "Brücke" ihrer Natur nach ein diskretes Element ist. Ausserdem versteht man, dass die Massenkonstante m des neutralen Teilchens notwendig positiv sein muss, da sich der Schwarzschild'schen Lösung für negatives m keine singularitätsfreie Lösung zuordnen lässt. Erst die Untersuchung des

Mehr-Brücken-Problems kann zeigen, ob diese theoretische Methode eine Erklärung für die empirisch erwiesene Massengleichheit der Teilchen in der Natur liefert, und ob sie den von der Quantenmechanik so wunderbar erfassten Tatsachen gerecht wird.

Analog lässt sich zeigen, dass die kombinierten Gleichungen von Gravitation und Elektrizität (bei passender Wahl des Vorzeichens des elektrischen Gliedes in den Gravitationsgleichungen) eine singularitätsfreie Brückendarstellung für das elektrische Korpuskel liefern. Die einfachste Lösung dieser Art ist die für ein elektrisches Teilchen ohne Gravitationsmasse.

Solange die bedeutenden mathematischen Schwierigkeiten für die Lösung des Mehr-Brücken-Problems nicht überwunden sind, lässt sich über die Brauchbarkeit der Theorie vom physikalischen Standpunkt aus nichts aussagen. Es ist aber tatsächlich der erste Versuch einer konsequenten Ausbildung einer Feldtheorie, von dem die Möglichkeit besteht, dass er die Eigenschaften der Materie darstellt. Zugunsten dieses Versuches muss auch geltend gemacht werden, dass er sich auf die nach dem heutigen Stand unserer mathematischen Einsicht einfachsten allgemein relativistischen Feldgleichungen gründet.

ZUSAMMENFASSUNG.

Physik ist ein in Entwicklung begriffenes logisches Gedankensystem dessen Grundlage nicht durch eine induktive Methode aus den Erlebnissen herausdestilliert sondern nur durch freie Erfindung gewonnen werden kann. Die Berechtigung (Wahrheitswert) des Systems liegt in der Bewährung von Folgesätzen an den Sinneserlebnissen, wobei die Beziehung der letzteren zu ersteren nur intuitiv erfassbar ist. Die Entwicklung vollzieht sich in der Richtung wachsender Einfachheit des logischen Fundamentes. Um diesem Ziele näher zu kommen, müssen wir uns damit abfinden, dass die logische Grundlage immer erlebnisferner und der gedankliche Weg von den Grundlagen bis zu jenen Folgesätzen, welche ihr Korrelat in Sinneserlebnissen finden, immer beschwerlicher und länger wird.

Unser Ziel war, die Entwicklung der Grundbegriffe in ihrer Abhängigkeit von dem Erlebnismaterial und vom

Streben nach innerer Vervollkommnung des Systems so kurz als möglich zu skizzieren. Der gegenwärtige Stand sollte durch diese Betrachtung beleuchtet werden—wie er mir erscheint. (Historisch-schematische Darstellung muss persönlich gefärbt sein; dies ist unvermeidlich.)

Ich suche zu zeigen, wie die Begriffe körperliches Objekt, Raum, subjektive und objektive Zeit miteinander und mit dem Charakter des Erlebens zusammenhängen. In der klassischen Mechanik werden die Begriffe Raum und Zeit selbständig; der Begriff des körperlichen Objekts wird in der Grundlage ersetzt durch den des materiellen Punktes, wodurch die Mechanik zum Fundament atomistisch wird. Bei dem Versuch, die Mechanik zum Fundament der ganzen Physik zu machen, machen Licht und Elektrizität unüberwindliche Schwierigkeiten. Dies führt zur Feldtheorie der Elektrizität und weiter zu dem Versuch, die Physik ganz auf den Feldbegriff zu gründen (nach versuchtem Kompromiss mit der klassischen Mechanik). Dieser Versuch führt zur Relativitätstheorie (Sublimierung des Raum- und Zeitbegriffes zum Kontinuum mit metrischer Struktur).

Ich suche ferner darzutun, warum nach meiner Ansicht die Quantentheorie nicht dazu geeignet zu sein scheint, ein brauchbares Fundament der Physik zu liefern: Man gerät in Widersprüche, wenn man die quantentheoretische Beschreibung als eine *vollständige* Beschreibung des einzelnen physikalischen Systems bezw. Vorgangs zu betrachten sucht.

Andererseits ist die Feldtheorie bisher nicht imstande, eine Theorie der Molekularstruktur der Materie und der Quanten-Phänomene zu liefern. Es wird aber gezeigt, dass die Ueberzeugung von der Unfähigkeit der Feldtheorie, diese Probleme mit ihren Methoden zu lösen, auf Vorurteilen beruht.

Photo Cells Used in Power Plant.—C. E. MILLER. (*Power*, Vol. 79, No. 12.) Industrial applications of photo cells have been many and varied, but their use in steam plants is relatively new. In the Buzzard Point plant of the Potomac Electric Power Co. there are installed pulverized fuel fired boilers. In operation of boilers of this type, particularly at very low ratings, it is important that the operator know instantly when ignition fails. Photo electric cells installed in each boiler at this plant so arranged that the source of illumination is from the furnace fire, operate an alarm if the fire goes out. In this way the operator is immediately notified and he can either cut in the gas lighters to relight the coal or cut off the coal feeders. Thus, unburned pulverized coal is prevented from being blown into the boiler setting. Another application of photo cells in this plant is on the circulating water intake screens in which clogging constitutes an ever present hazard. To obviate the necessity of having an attendant at the screens, and the possibility of an emergency shut down due to circulating water failure, an arrangement was adopted in which a float in the water on the inlet side of the revolving screens moves a photo electric cell. Water level on the discharge side moves a 110 volt lamp attached to another float. As the tide changes, the light source and photo electric cell move up and down with no change in their relative positions. If the screens become clogged, the water level on the discharge side will drop and raise the light beam off the photo cell. This causes, through relays, the starting up of a screen wash pump and the sounding of an alarm.

R. H. O.

"The highest form of research, whether its intent classifies it as pure research or applied, is not that which is concerned with things as they are, but that which is aimed clear out beyond existing things to something altogether new."—T. A. Boyd, "Research."

PHYSICS AND REALITY.

BY

ALBERT EINSTEIN.

(Translation by Jean Piccard.)

§ 1. GENERAL CONSIDERATION CONCERNING THE METHOD OF SCIENCE.

It has often been said, and certainly not without justification, that the man of science is a poor philosopher. Why then should it not be the right thing for the physicist to let the philosopher do the philosophizing? Such might indeed be the right thing at a time when the physicist believes he has at his disposal a rigid system of fundamental concepts and fundamental laws which are so well established that waves of doubt can not reach them; but, it can not be right at a time when the very foundations of physics itself have become problematic as they are now. At a time like the present, when experience forces us to seek a newer and more solid foundation, the physicist cannot simply surrender to the philosopher the critical contemplation of the theoretical foundations; for, he himself knows best, and feels more surely where the shoe pinches. In looking for a new foundation, he must try to make clear in his own mind just how far the concepts which he uses are justified, and are necessities.

The whole of science is nothing more than a refinement of every day thinking. It is for this reason that the critical thinking of the physicist cannot possibly be restricted to the examination of the concepts of his own specific field. He cannot proceed without considering critically a much more difficult problem, the problem of analyzing the nature of everyday thinking.

On the stage of our subconscious mind appear in colorful succession sense experiences, memory pictures of them, representations and feelings. In contrast to psychology, physics treats directly only of sense experiences and of the " understanding " of their connection. But even the concept of the

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" real external world " of everyday thinking rests exclusively on sense impressions.

Now we must first remark that the differentiation between sense impressions and representations is not possible; or, at least it is not possible with absolute certainty. With the discussion of this problem, which affects also the notion of reality, we will not concern ourselves but we shall take the existence of sense experiences as given, that is to say as psychic experiences of special kind.

I believe that the first step in the setting of a " real external world " is the formation of the concept of bodily objects and of bodily objects of various kinds. Out of the multitude of our sense experiences we take, mentally and arbitrarily, certain repeatedly occurring complexes of sense impression (partly in conjunction with sense impressions which are interpreted as signs for sense experiences of others), and we attribute to them a meaning—the meaning of the bodily object. Considered logically this concept is not identical with the totality of sense impressions referred to; but it is an arbitrary creation of the human (or animal) mind. On the other hand, the concept owes its meaning and its justification exclusively to the totality of the sense impressions which we associate with it.

The second step is to be found in the fact that, in our thinking (which determines our expectation), we attribute to this concept of the bodily object a significance, which is to a high degree independent of the sense impression which originally gives rise to it. This is what we mean when we attribute to the bodily object " a real existence." The justification of such a setting rests exclusively on that fact that, by means of such concepts and mental relations between them, we are able to orient ourselves in the labyrinth of sense impressions. These notions and relations, although free statements of our thoughts, appear to us as stronger and more unalterable than the individual sense experience itself, the character of which as anything other than the result of an illusion or hallucination is never completely guaranteed. On the other hand, these concepts and relations, and indeed the setting of real objects and, generally speaking, the existence of " the real world," have justification only in so far as they are connected with

sense impressions between which they form a mental connection.

The very fact that the totality of our sense experiences is such that by means of thinking (operations with concepts, and the creation and use of definite functional relations between them, and the coördination of sense experiences to these concepts) it can be put in order, this fact is one which leaves us in awe, but which we shall never understand. One may say "the eternal mystery of the world is its comprehensibility." It is one of the great realisations of Immanuel Kant that the setting up of a real external world would be senseless without this comprehensibility.

In speaking here concerning "comprehensibility," the expression is used in its most modest sense. It implies: the production of some sort of order among sense impressions, this order being produced by the creation of general concepts, relations between these concepts, and by relations between the concepts and sense experience, these relations being determined in any possible manner. It is in this sense that the world of our sense experiences is comprehensible. The fact that it is comprehensible is a miracle.

In my opinion, nothing can be said concerning the manner in which the concepts are to be made and connected, and how we are to coördinate them to the experiences. In guiding us in the creation of such an order of sense experiences, success in the result is alone the determining factor. All that is necessary is *the statement* of a set of rules, since without such rules the acquisition of knowledge in the desired sense would be impossible. One may compare these rules with the rules of a game in which, while the rules themselves are arbitrary, it is their rigidity alone which makes the game possible. However, the fixation will never be final. It will have validity only for a special field of application (i.e. there are no final categories in the sense of Kant).

The connection of the elementary concepts of every day thinking with complexes of sense experiences can only be comprehended intuitively and it is unadaptable to scientifically logical fixation. The totality of these connections,—none of which is expressible in notional terms,—is the only thing which differentiates the great building which is science from a

logical but empty scheme of concepts. By means of these connections, the purely notional theorems of science become statements about complexes of sense experiences.

We shall call " primary concepts " such concepts as are directly and intuitively connected with typical complexes of sense experiences. All other notions are—from the physical point of view—possessed of meaning, only in so far as they are connected, by theorems, with the primary notions. These theorems are partially definitions of the concepts (and of the statements derived logically from them) and partially theorems not derivable from the definitions, which express at least indirect relations between the " primary concepts," and in this way between sense experiences. Theorems of the latter kind are " statements about reality " or laws of nature, i.e. theorems which have to show their usefulness when applied to sense experiences comprehended by primary concepts. The question as to which of the theorems shall be considered as definitions and which as natural laws will depend largely upon the chosen representation. It really becomes absolutely necessary to make this differentiation only when one examines the degree to which the whole system of concepts considered is not empty from the physical point of view.

Stratification of the Scientific System.

The aim of science is, on the one hand, a comprehension, as *complete* as possible, of the connection between the sense experiences in their totality, and, on the other hand, the accomplishment of this aim *by the use of a minimum of primary concepts and relations*. (Seeking, as far as possible, logical unity in the world picture, i.e. paucity in logical elements.)

Science concerns the totality of the primary concepts, i.e. concepts directly connected with sense experiences, and theorems connecting them. In its first stage of development, science does not contain anything else. Our everyday thinking is satisfied on the whole with this level. Such a state of affairs cannot, however, satisfy a spirit which is really scientifically minded; because, the totality of concepts and relations obtained in this manner is utterly lacking in logical unity. In order to supplement this deficiency, one invents a system poorer in concepts and relations, a system

retaining the primary concepts and relations of the " first layer " as logically derived concepts and relations. This new " secondary system " pays for its higher logical unity by having, as its own elementary concepts (concepts of the second layer), only those which are no longer directly connected with complexes of sense experiences. Further striving for logical unity brings us to a tertiary system, still poorer in concepts and relations, for the deduction of the concepts and relations of the secondary (and so indirectly of the primary) layer. Thus the story goes on until we have arrived at a system of the greatest conceivable unity, and of the greatest poverty of concepts of the logical foundations, which are still compatible with the observation made by our senses. We do not know whether or not this ambition will ever result in a definite system. If one is asked for his opinion, he is inclined to answer no. While wrestling with the problems, however, one will never give up the hope that this greatest of all aims can really be attained to a very high degree.

An adherent to the theory of abstraction or induction might call our layers " degrees of abstraction "; but, I do not consider it justifiable to veil the logical independence of the concept from the sense experiences. The relation is not analogous to that of soup to beef but rather of wardrobe number to overcoat.

The layers are furthermore not clearly separated. It is not even absolutely clear which concepts belong to the primary layer. As a matter of fact, we are dealing with freely formed concepts, which, with a certainty sufficient for practical use, are intuitively connected with complexes of sense experiences in such a manner that, in any given case of experience, there is no uncertainty as to the applicability or non-applicability of the statement. The essential thing is the aim to represent the multitude of concepts and theorems, close to experience, as theorems, logically deduced and belonging to a basis, as narrow as possible, of fundamental concepts and fundamental relations which themselves can be chosen freely (axioms). The liberty of choice, however, is of a special kind; it is not in any way similar to the liberty of a writer of fiction. Rather, it is similar to that of a man engaged in solving a well designed word puzzle. He may, it is true, propose any word

as the solution; but, there is only *one* word which really solves the puzzle in all its forms. It is an outcome of faith that nature — as she is perceptible to our five senses — takes the character of such a well formulated puzzle. The successes reaped up to now by science do, it is true, give a certain encouragement for this faith.

The multitude of layers discussed above corresponds to the several stages of progress which have resulted from the struggle for unity in the course of development. As regards the final aim, intermediary layers are only of temporary nature. They must eventually disappear as irrelevant. We have to deal, however, with the science of today, in which these strata represent problematic partial successes which support one another but which also threaten one another, because today's systems of concepts contain deep seated incongruities which we shall meet later on.

It will be the aim of the following lines to demonstrate what paths the constructive human mind has entered, in order to arrive at a basis of physics which is logically as uniform as possible.

§ 2. MECHANICS AND THE ATTEMPTS TO BASE ALL PHYSICS UPON IT.

An important property of our sense experiences, and, more generally, of all of our experience, is its time-like order. This kind of order leads to the mental conception of a subjective time, an ordinating scheme for our experience. The subjective time leads then through the concept of the bodily object and of space, to the concept of objective time, as we shall see later on.

Ahead of the notion of objective time there is, however, the concept of space; and, ahead of the latter we find the concept of the bodily object. The latter is directly connected with complexes of sense experiences. It has been pointed out that one property which is characteristic of the notion " bodily object " is the property which provides that we coördinate to it an existence, independent of (subjective) time, and independent of the fact that it is perceived by our senses. We do this in spite of the fact that we perceive temporal alterations in it. Poincaré has justly emphasized the fact that we distinguish two kinds of alterations of the

bodily object, " changes of state " and " changes of position." The latter, he remarked, are alterations which we can reverse by arbitrary motions of our bodies.

That there are bodily objects to which we have to ascribe, within a certain sphere of perception, no alteration of state, but only alterations of position, is a fact of fundamental importance for the formation of the concept of space (in a certain degree even for the justification of the notion of the bodily object itself). Let us call such an object " practically rigid."

If, as the object of our perception, we consider simultaneously (i.e. as a single unit) two practically rigid bodies, then there exist for this ensemble such alterations as can *not* possibly be considered as changes of position of the whole, notwithstanding the fact that this is the case for each one of the two constituents. This leads to the notion of " change of relative position " of the two objects; and, in this way also to the notion of " relative position " of the two objects. It is found moreover that among the relative positions, there is one of a specific kind which we designate as " Contact." [1] Permanent contact of two bodies in three or more " points " means that they are united as a quasi rigid compound body. It is permissible to say that the second body forms then a (quasi rigid) continuation on the first body and may, in its turn, be continued quasi rigidly. The possibility of the quasi rigid continuation of a body is unlimited. The real essence of the conceivable quasi rigid continuation of a body B_0 is the infinite " space " determined by it.

In my opinion, the fact that every bodily object situated in any arbitrary manner can be put into contact with the quasi rigid continuation of a predetermined and chosen body B_0 (body of relation), this fact is the empirical basis of our conception of space. In pre-scientific thinking, the solid earth's crust plays the rôle of B_0 and its continuation. The very name geometry indicates that the concept of space is psychologically connected with the earth as an assigned body.

The bold notion of " space " which preceded all scientific

[1] It is in the nature of things that we are able to talk about these objects only by means of concepts of our own creation, concepts which themselves are not subject to definition. It is essential, however, that we make use only of such concepts concerning whose coördination to our experience we feel no doubt.

geometry transformed our mental concept of the relations of positions of bodily objects into the notion of the position of these bodily objects in "space." This, of itself, represents a great formal simplification. Through this concept of space one reaches, moreover, an attitude in which any description of position is admittedly a description of contact; the statement that a point of a bodily object is located at a point P of space means that the object touches the point P of the standard body of reference B_0 (supposed appropriately continued) at the point considered.

In the geometry of the Greeks, space plays only a qualitative rôle, since the position of bodies in relation to space is considered as given, it is true, but is not described by means of numbers. Descartes was the first to introduce this method. In his language, the whole content of Euclidian geometry can axiomatically be founded upon the following statements: (1) Two specified points of a rigid body determine a distance. (2) We may coördinate triplets of numbers X_1, X_2, X_3, to points of space in such a manner that for every distance $P' - P''$ under consideration, the coördinates of whose end points are X_1', X_2', X_3'; X_1'', X_2'', X_3'', the expression

$$S^2 = (X_1'' - X_1')^2 + (X_2'' - X_2')^2 + (X_3'' - X_3')^2$$

is independent of the position of the body, and of the positions of any and all other bodies.

The (positive) number S means the length of the stretch, or the distance between the two points P' and P'' of space (which are coincident with the points P' and P'' of the stretch).

The formulation is chosen, intentionally, in such a way that it expresses clearly, not only the logical and axiomatic, but also the empirical content of Euclidian geometry. The purely logical (axiomatic) representation of Euclidian geometry has, it is true, the advantage of greater simplicity and clarity. It pays for this, however, by renouncing representation of the connection between the notional construction and the sense experience upon which connection, alone, the significance of geometry for physics rests. The fatal error that the necessity of thinking, preceding all experience, was at the basis of Euclidian geometry and the concept of space

belonging to it, this fatal error arose from the fact that the empirical basis, on which the axiomatic construction of Euclidian geometry rests, had fallen into oblivion.

In so far as one can speak of the existence of rigid bodies in nature, Euclidian geometry is a physical science, the usefulness of which must be shown by application to sense experiences. It relates to the totality of laws which must hold for the relative positions of rigid bodies independently of time. As one may see, the physical notion of space also, as originally used in physics, is tied to the existence of rigid bodies.

From the physicist's point of view, the central importance of Euclidian geometry rests in the fact that its laws are independent of the specific nature of the bodies whose relative positions it discusses. Its formal simplicity is characterized by the properties of homogeneity and isotropy (and the existence of similar entities).

The concept of space is, it is true, useful, but not indispensable for geometry proper, i.e. for the formulation of rules about the relative positions of rigid bodies. In opposition to this, the concept of objective time, without which the formulation of the fundamentals of classical mechanics is impossible, is linked with the concept of the spacial continuum.

The introduction of objective time involves two statements which are independent of each other.

(1) The introduction of the objective local time by connecting the temporal sequence of experiences with the indications of a "clock," i.e. of a closed system with periodical occurrence.

(2) The introduction of the notion of objective time for the happenings in the whole space, by which notion alone the idea of local time is enlarged to the idea of time in physics.

Note concerning (1). As I see it, it does not mean a "petitio principii" if one puts the concept of periodical occurrence ahead of the concept of time, while one is concerned with the clarification of the origin and of the empirical content of the concept of time. Such a conception corresponds exactly to the precedence of the concept of the rigid (or quasi rigid) body in the interpretation of the concept of space.

Further discussion of (2). The illusion which prevailed

prior to the enunciation of the theory of relativity—that, from the point of view of experience the meaning of simultaneity in relation to happenings distant in space and consequently that the meaning of time in physics is a priori clear,—this illusion had its origin in the fact that in our everyday experience, we can neglect the time of propagation of light. We are accustomed on this account to fail to differentiate between "simultaneously seen" and "simultaneously happening"; and, as a result the difference between time and local time fades away.

The lack of definiteness which, from the point of view of empirical importance, adheres to the notion of time in classical mechanics was veiled by the axiomatic representation of space and time as things given independently of our senses. Such a use of notions—independent of the empirical basis, to which they owe their existence—does not necessarily damage science. One may however easily be led into the error of believing that these notions, whose origin is forgotten, are necessary and unalterable accompaniments to our thinking, and this error may constitute a serious danger to the progress of science.

It was fortunate for the development of mechanics and hence also for the development of physics in general, that the lack of definiteness in the concept of objective time remained obscured from the earlier philosophers as regards its empirical interpretation. Full of confidence in the real meaning of the space-time construction they developed the foundations of mechanics which we shall characterize, schematically, as follows:

(*a*) Concept of a material point: a bodily object which—as regards its position and motion—can be described with sufficient exactness as a point with coördinates X_1, X_2, X_3. Description of its motion (in relation to the "space" B_0) by giving X_1, X_2, X_3, as functions of the time.

(*b*) Law of inertia: the disappearance of the components of acceleration for the material point which is sufficiently far away from all other points.

(*c*) Law of motion (for the material point): Force = mass $\times$ acceleration.

(*d*) Laws of force (actions and reactions between material points).

In this (*b*) is nothing more than an important special case of (*c*). A real theory exists only when the laws of force are given. The forces must in the first place only obey the law of equality of action and reaction in order that a system of points—permanently connected to each other—may behave like *one* material point.

These fundamental laws, together with Newton's law for gravitational force, form the basis of the mechanics of celestial bodies. In this mechanics of Newton, and in contrast to the above conceptions of space derived from rigid bodies, the space B_0 enters in a form which contains a new idea; it is not for every B_0 that validity is required (for a given law of force) by (*b*) and (*c*), but only for a B_0 in the appropriate condition of motion (inertial system). On account of this fact, the coordinate space acquired an independent physical property which is not contained in the purely geometrical notion of space, a circumstance which gave Newton considerable food for thought (Experiment of Eimer).[2]

Classical mechanics is only a general scheme; it becomes a theory only by explicit indication of the force laws (*d*) as was done so very successfully by Newton for celestial mechanics. From the point of view of the aim of the greatest logical simplicity of the foundations, this theoretical method is deficient in so far as the laws of force cannot be obtained by logical and formal considerations, so that their choice is *a priori* to a large extent arbitrary. Also Newton's gravitation law of force is distinguished from other conceivable laws of force exclusively by its *success*.

In spite of the fact that, today, we know positively that classical mechanics fails as a foundation dominating all physics, it still occupies the center of all of our thinking in physics. The reason for this lies in the fact that, regardless of important progress reached since the time of Newton, we have not yet arrived at a new foundation of physics concerning

[2] This defect of the theory could only be eliminated by such a formulation of mechanics as would command validity for all B_0. This is one of the steps which lead to the general theory of relativity. A second defect, also eliminated only by the introduction of the general theory of relativity, lies in the fact that there is no reason given by mechanics itself for the equality of the gravitational and inertial mass of the material point.

which we may be certain that the whole complexity of investigated phenomena, and of partial theoretical systems of a successful kind, could be deduced logically from it. In the following lines I shall try to describe briefly how the matter stands.

First we try to get clearly in our minds how far the system of classical mechanics has shown itself adequate to serve as a basis for the whole of physics. Since we are dealing here only with the foundations of physics and with its development, we need not concern ourselves with the purely *formal* progresses of mechanics (equation of Lagrange, canonical equations etc.). *One* remark, however, appears indispensable. The notion " material point " is fundamental for mechanics. If now we seek the mechanics of a bodily object which itself can *not* be treated as a material point—and strictly speaking every object " perceptible to our senses " is of this category—then the question arises: How shall we imagine the object to be built up out of material points, and what forces must we assume as acting between them? The formulation of this question is indispensable, if mechanics is to pretend to describe the object *completely*.

It is natural to the tendency of mechanics to assume these material points, and the laws of forces acting between them, as invariable, since time alterations would lie outside of the scope of mechanical explanation. From this we can see that classical mechanics must lead us to an atomistic construction of matter. We now realize, with special clarity, how much in error are those theorists who believe that theory comes inductively from experience. Even the great Newton could not free himself from this error (" Hypotheses non fingo ").*

In order to save itself from becoming hopelessly lost in this line of thought (atomistic), science proceeded first in the following manner. The mechanics of a system is determined if its potential energy is given as a function of its configuration. Now, if the acting forces are of such a kind as to guarantee maintenance of certain qualities of order of the system's configuration, then the configuration may be described with sufficient accuracy by a relatively small number of configuration variables q_r; the potential energy is considered only insofar

* "I make no hypotheses."

as it is dependent upon *these* variables (for instance, description of the configuration of a practically rigid body by six variables).

A second method of application of mechanics, which avoids the consideration of a subdivision of matter down to " real " material points, is the mechanics of so-called continuous media. This mechanics is characterized by the fiction that the density of matter and speed of matter is dependent in a continuous manner upon coördinates and time, and that the part of the interactions not explicitly given can be considered as surface forces (pressure forces) which again are continuous functions of location. Herein we find the hydrodynamic theory, and the theory of elasticity of solid bodies. These theories avoid the explicit introduction of material points by fictions which, in the light of the foundation of classical mechanics, can only have an approximate significance.

In addition to their great *practical* significance, these categories of science have—by enlargement of the mathematical world of ideas—created those formal auxiliary instruments (partial differential equations) which have been necessary for the subsequent attempts at formulating the total scheme of physics in a manner which is new as compared with that of Newton.

These two modes of application of mechanics belong to the so-called " phenomenological " physics. It is characteristic of this kind of physics that it makes as much use as possible of concepts which are close to experience but which, for this reason, have to give up, to a large degree, unity in the foundations. Heat, electricity and light are described by special variables of state and constants of matter other than the mechanical state; and to determine all of these variables in their relative dependence was a rather empirical task. Many contemporaries of Maxwell saw in such a manner of presentation the ultimate aim of physics, which they thought could be obtained purely inductively from experience on account of the relative closeness of the concepts used to the experience. From the point of view of theories of knowledge St. Mill and E. Mach took their stand approximately on this ground.

According to my belief, the greatest achievement of Newton's mechanics lies in the fact that its consistent applica-

tion has led beyond this phenomenological representation, particularly in the field of heat phenomena. This occurred in the kinetic theory of gases and, in a general way, in statistical mechanics. The former connected the equation of state of the ideal gases, viscosity, diffusion and heat conductivity of gases and radiometric phenomena of gases, and gave the logical connection of phenomena which, from the point of view of direct experience, had nothing whatever to do with one another. The latter gave a mechanical interpretation of the thermodynamic ideas and laws as well as the discovery of the limit of applicability of the notions and laws to the classical theory of heat. This kinetic theory which surpassed, by far, the phenomenological physics as regards the logical unity of its foundations, produced moreover definite values for the true magnitudes of atoms and molecules which resulted from several independent methods and were thus placed beyond the realm of reasonable doubt. These decisive progresses were paid for by the coördination of atomistic entities to the material points, the constructively speculative character of which entities being obvious. Nobody could hope ever to "perceive directly" an atom. Laws concerning variables connected more directly with experimental facts (for example: temperature, pressure, speed) were deduced from the fundamental ideas by means of complicated calculations. In this manner physics (at least part of it), originally more phenomenologically constructed, was reduced, by being founded upon Newton's mechanics for atoms and molecules, to a basis further removed from direct experiment, but more uniform in character.

§ 3. THE FIELD CONCEPT.

In explaining optical and electrical phenomena Newton's mechanics has been far less successful than it had been in the fields cited above. It is true that Newton tried to reduce light to the motion of material points in his corpuscular theory of light. Later on, however, as the phenomena of polarization, diffraction and interference of light forced upon his theory more and more unnatural modifications, Huyghens' undulatory theory of light, prevailed. Probably this theory owes its origin essentially to the phenomena of crystallographic optics and to the theory of sound, which was then

already elaborated to a certain degree. It must be admitted that Huyghens' theory also was based in the first instance upon classical mechanics; but, the all-penetrating ether had to be assumed as the carrier of the waves and the structure of the ether, formed from material points, could not be explained by any known phenomenon. One could never get a clear picture of the interior forces governing the ether, nor of the forces acting between the ether and the "ponderable" matter. The foundations of this theory remained, therefore, eternally in the dark. The true basis was a partial differential equation, the reduction of which to mechanical elements remained always problematic.

For the theoretical conception of electric and magnetic phenomena one introduced, again, masses of a special kind, and between these masses one assumed the existence of forces acting at a distance, similar to Newton's gravitational forces. This special kind of matter, however, appeared to be lacking in the fundamental property of inertia; and, the forces acting between these masses and the ponderable matter remained obscure. To these difficulties there had to be added the polar character of these kinds of matter which did not fit into the scheme of classical mechanics. The basis of the theory became still more unsatisfactory when electrodynamic phenomena became known, notwithstanding the fact that these phenomena brought the physicist to the explanation of magnetic phenomena through electrodynamic phenomena and, in this way, made the assumption of magnetic masses superfluous. This progress had, indeed, to be paid for by increasing the complexity of the forces of interaction which had to be assumed as existing between electrical masses in motion.

The escape from this unsatisfactory situation by the electric field theory of Faraday and Maxwell represents probably the most profound transformation which has been experienced by the foundations of physics since Newton's time. Again, it has been a step in the direction of constructive speculation which has increased the distance between the foundation of the theory and what can be experienced by means of our five senses. The existence of the field manifests itself, indeed, only when electrically charged bodies are introduced into it. The differential equations of Maxwell connect the spacial and

temporal differential coefficients of the electric and magnetic fields. The electric masses are nothing more than places of non-disappearing divergency of the electric field. Light waves appear as undulatory electromagnetic field processes in space.

To be sure, Maxwell still tried to interpret his field theory mechanically by means of mechanical ether models. But these attempts receded gradually to the background following the representation—purged of any unnecessary additions—by Heinrich Hertz, so that, in this theory the field finally took the fundamental position which had been occupied in Newton's mechanics by the material points. At first, however, this applies only for electromagnetic fields in empty space.

In its initial stage the theory was yet quite unsatisfactory for the interior of matter, because there, two electric vectors had to be introduced, which were connected by relations dependent on the nature of the medium, these relations being inaccessible to any theoretical analysis. An analogous situation arose in connection with the magnetic field, as well as in the relation between electric current density and the field.

Here H. A. Lorentz found an escape which showed, at the same time, the way to an electrodynamic theory of bodies in motion, a theory which was more or less free of arbitrary assumption. His theory was built on the following fundamental hypothesis:

Everywhere (including the interior of ponderable bodies) the seat of the field is the empty space. The participation of matter in electromagnetic phenomena has its origin only in the fact that the elementary particles of matter carry unalterable electric charges, and, on this account are subject on the one hand to the actions of ponderomotive forces and on the other hand possess the property of generating a field. The elementary particles obey Newton's law of motion for the material point.

This is the basis on which H. A. Lorentz obtained his synthesis of Newton's mechanics and Maxwell's field theory. The weakness of this theory lies in the fact that it tried to determine the phenomena by a combination of partial differential equations (Maxwell's field equations for empty space) and total differential equations (equations of motion of

points), which procedure was obviously unnatural. The unsatisfactory part of the theory showed up externally by the necessity of assuming finite dimensions for the particles in order to prevent the electromagnetic field existing at their surfaces from becoming infinitely great. The theory failed moreover to give any explanation concerning the tremendous forces which hold the electric charges on the individual particles. H. A. Lorentz accepted these weaknesses of his theory, which were well known to him, in order to explain the phenomena correctly at least as regards their general lines.

Furthermore, there was one consideration which reached beyond the frame of Lorentz's theory. In the environment of an electrically charged body there is a magnetic field which furnishes an (apparent) contribution to its inertia. Should it not be possible to explain the *total* inertia of the particles electromagnetically? It is clear that this problem could be worked out satisfactorily only if the particles could be interpreted as regular solutions of the electromagnetic partial differential equations. The Maxwell equations in their original form do not, however, allow such a description of particles, because their corresponding solutions contain a singularity. Theoretical physicists have tried for a long time, therefore, to reach the goal by a modification of Maxwell's equations. These attempts have, however, not been crowned with success. Thus it happened that the goal of erecting a pure electromagnetic field theory of matter remained unattained for the time being, although in principle no objection could be raised against the possibility of reaching such a goal. The thing which deterred one in any further attempt in this direction was the lack of any systematic method leading to the solution. What appears certain to me, however, is that, in the foundations of any consistent field theory, there shall not be, in addition to the concept of field, any concept concerning particles. The whole theory must be based solely on partial differential equations and their singularity-free solutions.

§ 4. THE THEORY OF RELATIVITY.

There is no inductive method which could lead to the fundamental concepts of physics. Failure to understand this fact constituted the basic philosophical error of so many in-

vestigators of the nineteenth century. It was probably the reason why the molecular theory, and Maxwell's theory were able to establish themselves only at a relatively late date. Logical thinking is necessarily deductive; it is based upon hypothetical concepts and axioms. How can we hope to choose the latter in such a manner as to justify us in expecting success as a consequence?

The most satisfactory situation is evidently to be found in cases where the new fundamental hypotheses are suggested by the world of experience itself. The hypothesis of the non-existence of perpetual motion as a basis for thermodynamics affords such an example of a fundamental hypothesis suggested by experience; the same thing holds for the principle of inertia of Gallileo. In the same category, moreover, we find the fundamental hypotheses of the theory of relativity, which theory has led to an unexpected expansion and broadening of the field theory, and to the superceding of the foundations of classical mechanics.

The successes of the Maxwell-Lorentz theory have given great confidence in the validity of the electromagnetic equations for empty space and hence, in particular, to the statement that light travels " in space " with a certain constant speed c. Is this law of the invariability of light velocity in relation to any desired inertial system valid? If it were not, then one specific inertial system or more accurately, one specific state of motion (of a body of reference), would be distinguished from all others. In opposition to this idea, however, stand all the mechanical and electromagnetic-optical facts of our experience.

For these reasons it was necessary to raise to the degree of a principle, the validity of the law of constancy of light velocity for all inertial systems. From this, it follows that the spacial coördinates X_1, X_2, X_3, and the time X_4, must be transformed according to the " Lorentz-transformation " which is characterized by invariance of the expression

$$ds^2 = dx_1^2 + dx_2^2 + dx_3^2 - dx_4^2$$

(if the unit of time is chosen in such a manner that the speed of light $c = 1$).

By this procedure time lost its absolute character, and was

included with the " spacial " coördinates as of algebraically (nearly) similar character. The absolute character of time and particularly of simultaneity were destroyed, and the four dimensional description became introduced as the only adequate one.

In order to account, also, for the equivalence of all inertial systems with regard to all the phenomena of nature, it is necessary to postulate invariance of all systems of physical equations which express general laws, with regard to the Lorentzian transformation. The elaboration of this requirement forms the content of the special theory of relativity.

This theory is compatible with the equations of Maxwell; but, it is incompatible with the basis of classical mechanics. It is true that the equations of motion of the material point can be modified (and with them the expressions for momentum and kinetic energy of the material point) in such a manner as to satisfy the theory; but, the concept of the force of interaction, and with it the concept of potential energy of a system, lose their basis, because these concepts rest upon the idea of absolute instantaneousness. The field, as determined by differential equations, takes the place of the force.

Since the foregoing theory allows interaction only by fields, it requires a field theory of gravitation. Indeed, it is not difficult to formulate such a theory in which, as in Newton's theory, the gravitational fields can be reduced to a scalar which is the solution of a partial differential equation. However, the experimental facts expressed in Newton's theory of gravitation lead in another direction, that of the general theory of relativity.

Classical mechanics contains one point which is unsatisfactory in that, in the fundamentals, the same mass constant is met twice over in two different rôles, namely as " inertial mass " in the law of motion, and as " gravitational mass " in the law of gravitation. As a result of this, the acceleration of a body in a pure gravitational field is independent of its material; or, in a coördinate system of *uniform acceleration* (accelerated in relation to an " inertial system ") the motions take place as they would in a homogeneous gravitational field (in relation to a " motionless " system of coördinates). If one assumes that the equivalence of these two cases is com-

plete, then one attains an adaptation of our theoretical thinking to the fact that the gravitational and inertial masses are identical.

From this it follows that there is no longer any reason for favoring, as a fundamental principle, the " inertial systems "; and, we must admit as equivalent in their own right, also *non-linear* transformations of the coördinates (x_1, x_2, x_3, x_4). If we make such a transformation of a system of coördinates of the special theory of relativity, then the metric

$$ds^2 = dx_1^2 + dx_2^2 + dx_3^2 - dx_4^2$$

goes over to a general (Riemannian) metric of Bane

$$ds^2 = g_{\mu\nu}\, dx_\mu\, dx_\nu \quad \text{(Summed over } \mu \text{ and } \nu\text{)}$$

where the $g_{\mu\nu}$, symmetrical in μ and ν, are certain functions of $x_1 \cdots x_4$ which describe both the metric property, and the gravitational field in relation to the new system of coördinates.

The foregoing improvement in the interpretation of the mechanical basis must, however, be paid for in that—as becomes evident on closer scrutiny—the new coördinates could no longer be interpreted, as results of measurements by rigid bodies and clocks, as they could in the original system (an inertial system with vanishing gravitational field).

The passage to the general theory of relativity is realized by the assumption that such a representation of the field properties of space already mentioned, by functions $g_{\mu\nu}$ (that is to say by a Riemann metric), is also justified in the *general* case in which there is no system of coördinates in relation to which the metric takes the simple quasi-Euclidian form of the special theory of relativity.

Now the coördinates, by themselves, no longer express metric relations, but only the " neighborliness " of the things described, whose coördinates differ but little from one another. All transformations of the coördinates have to be admitted so long as these transformations are free from singularities. Only such equations as are covariant in relation to arbitrary transformations in this sense have meaning as expressions of general laws of nature (postulate of general covariancy).

The first aim of the general theory of relativity was a pre-

liminary statement which, by giving up the requirement of constituting a closed thing in itself, could be connected in as simple a manner as possible with the "facts directly observed." Newton's gravitational theory gave an example, by restricting itself to the pure mechanics of gravitation. This preliminary statement may be characterized as follows:

(1) The concept of the material point and of its mass is retained. A law of motion is given for it, this law of motion being the translation of the law of inertia into the language of the general theory of relativity. This law is a system of total differential equations, the system characteristic of the geodetic line.

(2) In place of Newton's law of interaction by gravitation, we shall find the system of the simplest generally covariant differential equations which can be set up for the $g_{\mu\nu}$–tensor. It is formed by equating to zero the once contracted Riemannian curvature tensor ($R_{\mu\nu} = 0$).

This formulation permits the treatment of the problem of the planets. More accurately speaking, it allows the treatment of the problem of motion of material points of practically negligible mass in the gravitational field produced by a material point which itself is supposed to have no motion (central symmetry). It does not take into account the reaction of the "moved" material points on the gravitational field, nor does it consider how the central mass produces this gravitational field.

Analogy with classical mechanics shows that the following is a way to complete the theory. One sets up as field equation

$$R_{ik} - \tfrac{1}{2}g_{ik}R = -T_{ik},$$

where R represents the scalar of Riemannian curvature, T_{ik} the energy tensor of the matter in a phenomenological representation. The left side of the equation is chosen in such a manner that its divergence disappears identically. The resulting disappearance of the divergence of the right side produces the "equations of motion" of matter, in the form of partial differential equations for the case where T_{ik} introduces, for the description of the matter, only *four* further functions independent of each other (for instance, density, pressure, and velocity components, where there is between

the latter an identity, and between pressure and density an equation of condition).

By this formulation one reduces the whole mechanics of gravitation to the solution of a single system of covariant partial differential equations. The theory avoids all internal discrepancies which we have charged against the basis of classical mechanics. It is sufficient—as far as we know—for the representation of the observed facts of celestial mechanics. But, it is similar to a building, one wing of which is made of fine marble (left part of the equation), but the other wing of which is built of low grade wood (right side of equation). The phenomenological representation of matter is, in fact, only a crude substitute for a representation which would correspond to all known properties of matter.

There is no difficulty in connecting Maxwell's theory of the electromagnetic field with the theory of the gravitational field so long as one restricts himself to space, free of ponderable matter and free of electric density. All that is necessary is to put on the right hand side of the above equation for T_{ik}, the energy tensor of the electromagnetic field in empty space and to associate with the so modified system of equations the Maxwell field equation for empty space, written in general covariant form. Under these conditions there will exist, between all these equations, a sufficient number of the differential identities to guarantee their consistency. We may add that this necessary formal property of the total system of equations leaves arbitrary the choice of the sign of the member T_{ik}, a fact which was later shown to be important.

The desire to have, for the foundations of the theory, the greatest possible unity has resulted in several attempts to include the gravitational field and the electromagnetic field in one formal but homogeneous picture. Here we must mention particularly the five-dimensional theory of Kaluza and Klein. Having considered this possibility very carefully I feel that it is more desirable to accept the lack of internal uniformity of the original theory, because I do not consider that the totality of the hypothetical basis of the five-dimensional theory contains less of an arbitrary nature than does the original theory. The same statement may be made for the projective variety of the theory, which has been elaborated with great care, in particular, by v. Dantzig and by Pauli.

The foregoing considerations concern, exclusively, the theory of the field, free of matter. How are we to proceed from this point in order to obtain a complete theory of atomically constructed matter? In such a theory, singularities must certainly be excluded, since without such exclusion the differential equations do not completely determine the total field. Here, in the field theory of general relativity, we meet the same problem of a theoretical field-representation of matter as was met originally in connection with the pure Maxwell theory.

Here again the attempt to construct particles out of the field theory, leads apparently to singularities. Here also the endeavor has been made to overcome this defect by the introduction of new field variables and by elaborating and extending the system of field equations. Recently, however, I discovered, in collaboration with Dr. Rosen, that the above mentioned simplest combination of the field equations of gravitation and electricity produces centrally symmetrical solutions which can be represented as free of singularity (the well known centrally symmetrical solutions of Schwarzschild for the pure gravitational field, and those of Reissner for the electric field with consideration of its gravitational action). We shall refer to this shortly in the paragraph next but one. In this way it seems possible to get for matter and its interactions a pure field theory free of additional hypotheses, one moreover whose test by submission to facts of experience does not result in difficulties other than purely mathematical ones, which difficulties, however, are very serious.

§ 5. QUANTUM THEORY AND THE FUNDAMENTALS OF PHYSICS.

The theoretical physicists of our generation are expecting the erection of a new theoretical basis for physics which would make use of fundamental concepts greatly different from those of the field theory considered up to now. The reason is that it has been found necessary to use—for the mathematical representation of the so-called quantum phenomena—new sorts of methods of consideration.

While the failure of classical mechanics, as revealed by the theory of relativity, is connected with the finite speed of light (its avoidance of being ∞), it was discovered at the beginning

of our century that there were other kinds of inconsistencies between deductions from mechanics and experimental facts, which inconsistencies are connected with the finite magnitude (the avoidance of being zero) of Planck's constant h. In particular, while molecular mechanics requires that both, heat content and (monochromatic) radiation density, of solid bodies should decrease *in proportion* to the decreasing absolute temperature, experience has shown that they decrease much more rapidly than the absolute temperature. For a theoretical explanation of this behavior it was necessary to assume that the energy of a mechanical system cannot assume any sort of value, but only certain discrete values whose mathematical expressions were always dependent upon Planck's constant h. Moreover, this conception was essential for the theory of the atom (Bohr's theory). For the transitions of these states into one another,—with or without emission or absorption of radiation,—no causal laws could be given, but only statistical ones; and, a similar conclusion holds for the radioactive decomposition of atoms, which decomposition was carefully investigated about the same time. For more than two decades physicists tried vainly to find a uniform interpretation of this "quantum character" of systems and phenomena. Such an attempt was successful about ten years ago, through the agency of two entirely different theoretical methods of attack. We owe one of these to Heisenberg and Dirac, and the other to de Broglie and Schrödinger. The mathematical equivalence of the two methods was soon recognized by Schrödinger. I shall try here to sketch the line of thought of de Broglie and Schrödinger, which lies closer to the physicist's method of thinking, and shall accompany the description with certain general considerations.

The question is first: How can one assign a discrete succession of energy value H_σ to a system specified in the sense of classical mechanics (the energy function is a given function of the coördinates q_τ and the corresponding momenta p_τ)? Planck's constant h relates the frequency H_σ/h to the energy values H_σ. It is therefore sufficient to give to the system a succession of discrete *frequency* values. This reminds us of the fact that in acoustics, a series of discrete frequency values is coördinated to a linear partial differential equation (if

boundary values are given) namely the sinusoidal periodic solutions. In corresponding manner, Schrödinger set himself the task of coördinating a partial differential equation for a scalar function ψ to the given energy function $\mathcal{E}(q_r, p_r)$, where the q_r and the time t are independent variables. In this he succeeded (for a complex function ψ) in such a manner that the theoretical values of the energy H_s, as required by the statistical theory, actually resulted in a satisfactory manner from the periodic solution of the equation.

To be sure, it did not happen to be possible to associate a definite movement, in the sense of mechanics of material points, with a definite solution $\psi(q_r, t)$ of the Schrödinger equation. This means that the ψ function does not determine, at any rate *exactly*, the story of the q_r as functions of the time t. According to Born, however, an interpretation of the physical meaning of the ψ functions was shown to be possible in the following manner: $\psi\bar{\psi}$ (the square of the absolute value of the complex function ψ) is the probability density at the point under consideration in the configuration-space of the q_r, at the time t. It is therefore possible to characterize the content of the Schrödinger equation in a manner, easy to be understood, but not quite accurate, as follows: it determines how the probability density of a statistical ensemble of systems varies in the configuration-space with the time. Briefly: the Schrödinger equation determines the alteration of the function ψ of the q_r with the time.

It must be mentioned that the result of this theory contains—as limiting values—the result of the particle mechanics if the wave-length encountered during the solution of the Schrödinger problem is everywhere so small that the potential energy varies by a practically infinitely small amount for a change of one wave-length in the configuration-space. Under these conditions the following can in fact be shown: We choose a region G_0 in the configuration-space which, although large (in every dimension) in relation to the wave length, is small in relation to the practical dimensions of the configuration-space. Under these conditions it is possible to choose a function of ψ for an initial time t_0 in such a manner that it vanishes outside of the region G_0, and behaves, according to the Schrödinger equation, in such a manner that it retains this

property—approximately at least—also for a later time, but with the region G_0 having passed at that time t into another region G. In this manner one can, with a certain degree of approximation, speak of the motion of the region G as a whole, and one can approximate this motion by the motion of a point in the configuration-space. This motion then coincides with the motion which is required by the equations of classical mechanics.

Experiments on interference made with particle rays have given a brilliant proof that the wave character of phenomena of motion as assumed by the theory does, really, correspond to the facts. In addition to this, the theory succeeded, easily, in demonstrating the statistical laws of the transition of a system from one quantum condition to another under the action of external forces, which, from the standpoint of classical mechanics, appears as a miracle. The external forces were here represented by small additions of the potential energy as functions of the time. Now, while in classical mechanics, such additions can produce only correspondingly small alterations of the system, in the quantum mechanics they produce alterations of any magnitude however large, but with correspondingly small probability, a consequence in perfect harmony with experience. Even an understanding of the laws of radioactive decomposition, at least in their broad lines, was provided by the theory.

Probably never before has a theory been evolved which has given a key to the interpretation and calculation of such a heterogeneous group of phenomena of experience as has the quantum theory. In spite of this, however, I believe that the theory is apt to beguile us into error in our search for a uniform basis for physics, because, in my belief, it is an *incomplete* representation of real things, although it is the only one which can be built out of the fundamental concepts of force and material points (quantum corrections to classical mechanics). The incompleteness of the representation is the outcome of the statistical nature (incompleteness) of the laws. I will now justify this opinion.

I ask first: How far does the ψ function describe a real condition of a mechanical system? Let us assume the ψ_r to be the periodic solutions (put in the order of increasing energy

values) of the Schrödinger equation. I shall leave open, for the time being, the question as to how far the individual ψ_r are *complete* descriptions of physical conditions. A system is first in the condition ψ_1 of lowest energy $\mathcal{E}_1$. Then during a finite time a small disturbing force acts upon the system. At a later instant one obtains then from the Schrödinger equation a ψ function of the form

$$\psi = \sum c_r \psi_r,$$

where the c_r are (complex) constants. If the ψ_r are "normalized," then $|c_1|$ is nearly equal to 1, $|c_2|$ etc. is small compared with 1. One may now ask: Does ψ describe a real condition of the system? If the answer is yes, then we can hardly do otherwise than ascribe [3] to this condition a definite energy $\mathcal{E}$, and, in particular, such an energy as exceeds $\mathcal{E}_1$ by a small amount (in any case $\mathcal{E}_1 < \mathcal{E} < \mathcal{E}_2$). Such an assumption is, however, at variance with the experiments on electron impact such as have been made by J. Franck and G. Hertz, if, in addition to this, one accepts Millikan's demonstration of the discrete nature of electricity. As a matter of fact, these experiments lead to the conclusion that energy values of a state lying between the quantum values do not exist. From this it follows that our function ψ does not in any way describe a homogeneous condition of the body, but represents rather a statistical description in which the c_r represent probabilities of the individual energy values. It seems to be clear, therefore, that the Born statistical interpretation of the quantum theory is the only possible one. The ψ function does not in any way describe a condition which could be that of a single system; it relates rather to many systems, to "an ensemble of systems" in the sense of statistical mechanics. If, except for certain special cases, the ψ function furnishes only *statistical* data concerning measurable magnitudes, the reason lies not only in the fact that the *operation of measuring* introduces unknown elements, which can be grasped only statistically, but because of the very fact that the ψ function does not, in any sense, describe the condition of *one* single system. The

[3] Because, according to a well established consequence of the relativity theory, the energy of a complete system (at rest) is equal to its inertia (as a whole). This, however, must have a well defined value.

Schrödinger equation determines the time variations which are experienced by the ensemble of systems which may exist with or without external action on the single system.

Such an interpretation eliminates also the paradox recently demonstrated by myself and two collaborators, and which relates to the following problem.

Consider a mechanical system constituted of two partial systems A and B which have interaction with each other only during limited time. Let the ψ function before their interaction be given. Then the Schrödinger equation will furnish the ψ function after the interaction has taken place. Let us now determine the physical condition of the partial system A as completely as possible by measurements. Then the quantum mechanics allows us to determine the ψ function of the partial system B from the measurements made, and from the ψ function of the total system. This determination, however, gives a result which depends upon *which* of the determining magnitudes specifying the condition of A has been measured (for instance coördinates *or* momenta). Since there can be only *one* physical condition of B after the interaction and which can reasonably not be considered as dependent on the particular measurement we perform on the system A separated from B it may be concluded that the ψ function is *not* unambiguously coördinated with the physical condition. This coördination of several ψ functions with the same physical condition of system B shows again that the ψ function cannot be interpreted as a (complete) description of a physical condition of a unit system. Here also the coördination of the ψ function to an ensemble of systems eliminates every difficulty.[4]

The fact that quantum mechanics affords, in such a simple manner, statements concerning (apparently) discontinuous transitions from one total condition to another without actually giving a representation of the specific process, this fact is connected with another, namely the fact that the theory, in reality, does not operate with the single system, but with a totality of systems. The coefficients c_r of our first example

[4] The operation of measuring A, for example, thus involves a transition to a narrower ensemble of systems. The latter (hence also its ψ function) depends upon the point of view according to which this narrowing of the ensemble of systems is made.

are really altered very little under the action of the external force. With this interpretation of quantum mechanics one can understand why this theory can easily account for the fact that weak disturbing forces are able to produce alterations of any magnitude in the physical condition of a system. Such disturbing forces produce, indeed, only correspondingly small alterations of the *statistical density* in the ensemble of systems, and hence only infinitely weak alterations of the ψ functions, the mathematical description of which offers far less difficulty than would be involved in the mathematical representation of finite alterations experienced by part of the single systems. What happens to the single system remains, it is true, entirely unclarified by this mode of consideration; this enigmatic happening is entirely eliminated from the representation by the statistical manner of consideration.

But now I ask: Is there really any physicist who believes that we shall never get any inside view of these important alterations in the single systems, in their structure and their causal connections, and this regardless of the fact that these single happenings have been brought so close to us, thanks to the marvelous inventions of the Wilson chamber and the Geiger counter? To believe this is logically possible without contradiction; but, it is so very contrary to my scientific instinct that I cannot forego the search for a more complete conception.

To these considerations we should add those of another kind which also voice their plea against the idea that the methods introduced by quantum mechanics are likely to give a useful basis for the whole of physics. In the Schrödinger equation, absolute time, and also the potential energy, play a decisive rôle, while these two concepts have been recognized by the theory of relativity as inadmissable in principle. If one wishes to escape from this difficulty he must found the theory upon field and field laws instead of upon forces of interaction. This leads us to transpose the statistical methods of quantum mechanics to fields, that is to systems of infinitely many degrees of freedom. Although the attempts so far made are restricted to linear equations, which, as we know from the results of the general theory of relativity, are insufficient, the complications met up to now by the very ingenious attempts are already terrifying. They certainly will rise

sky high if one wishes to obey the requirements of the general theory of relativity, the justification of which in principle nobody doubts.

To be sure, it has been pointed out that the introduction of a space-time continuum may be considered as contrary to nature in view of the molecular structure of everything which happens on a small scale. It is maintained that perhaps the success of the Heisenberg method points to a purely algebraical method of description of nature, that is to the elimination of continuous functions from physics. Then, however, we must also give up, by principle, the space-time continuum. It is not unimaginable that human ingenuity will some day find methods which will make it possible to proceed along such a path. At the present time, however, such a program looks like an attempt to breathe in empty space.

There is no doubt that quantum mechanics has seized hold of a beautiful element of truth, and that it will be a test stone for any future theoretical basis, in that it must be deducible as a limiting case from that basis, just as electrostatics is deducible from the Maxwell equations of the electromagnetic field or as thermodynamics is deducible from classical mechanics. However, I do not believe that quantum mechanics will be the *starting point* in the search for this basis, just as, vice versa, one could not go from thermodynamics (resp. statistical mechanics) to the foundations of mechanics.

In view of this situation, it seems to be entirely justifiable seriously to consider the question as to whether the basis of field physics cannot by *any* means be put into harmony with the facts of the quantum theory. Is this not the only basis which, consistently with today's possibility of mathematical expression, can be adapted to the requirements of the general theory of relativity? The belief, prevailing among the physicists of today, that such an attempt would be hopeless, may have its root in the unjustifiable idea that such a theory should lead, as a first approximation, to the equations of classical mechanics for the motion of corpuscles, or at least to total differential equations. As a matter of fact up to now we have never succeeded in representing corpuscles theoretically by fields free of singularities, and we can, a priori, say nothing about the behavior of such entities. *One thing,*

however, is certain: if a field theory results in a representation of corpuscles free of singularities, then the behavior of these corpuscles with time is determined solely by the differential equations of the field.

§ 6. RELATIVITY THEORY AND CORPUSCLES.

I shall now show that, according to the general theory of relativity, there exist singularity-free solutions of field equations which can be interpreted as representing corpuscles. I restrict myself here to neutral particles because, in another recent publication in collaboration with Dr. Rosen, I have treated this question in a detailed manner, and because the essentials of the problem can be completely shown by this case.

The gravitational field is entirely described by the tensor $g_{\mu\nu}$. In the three-index symbols $\Gamma_{\mu\nu}^{\sigma}$, there appear also the contravariants $g^{\mu\nu}$ which are defined as the minors of the $g_{\mu\nu}$ divided by the determinant $g(=|g_{\alpha\beta}|)$. In order that the R_{ik} shall be defined and finite, it is not sufficient that there shall be, for the environment of every part of the continuum, a system of coördinates in which the $g_{\mu\nu}$ and their first differential quotients are continuous and differentiable, but it is also necessary that the determinant g shall nowhere vanish. This last restriction is, however, eliminated if one replaces the differential equations $R_{ik} = 0$ by $g^2 R_{ik} = 0$, the left hand sides of which are *whole* rational functions of the g_{ik} and of their derivatives.

These equations have the centrally symmetrical solutions indicated by Schwarzschild

$$ds^2 = -\frac{1}{1-2m/r}dr^2 - r^2(d\theta^2 + \sin^2\theta d\varphi^2) + \left(1 - \frac{2m}{r}\right)dt^2.$$

This solution has a singularity at $r = 2m$, since the coefficient of dr^2 (i.e. g_{11}), becomes infinite on this hypersurface. If, however, we replace the variable r by ρ defined by the equation

$$\rho^2 = r - 2m$$

we obtain

$$ds^2 = -4(2m + \rho^2)d\rho^2 - (2m + \rho^2)^2(d\theta^2 + \sin^2\theta d\varphi^2) + \frac{\rho^2}{2m + \rho^2}dt^2.$$

This solution behaves regularly for all values of ρ. The vanishing of the coefficient of dt^2 i.e. (g_{44}) for $\rho = 0$ results, it is true, in the consequence that the determinant g vanishes for this value; but, with the methods of writing the field equations actually adopted, this does not constitute a singularity.

If ρ extends from $-\infty$ to $+\infty$, then r runs from $+\infty$ to $r = 2m$ and then back to $+\infty$, while for such values of r as correspond to $r < 2m$ there are no corresponding real values of ρ. Hence the Schwarzschild solution becomes a regular solution by representation of the physical space as consisting of two identical " shells " neighboring upon the hypersurface $\rho = 0$, that is $r = 2m$, while for this hypersurface the determinant g vanishes. Let us call such a connection between the two (identical) shells a " bridge." Hence the existence of such a bridge between the two shells in the finite realm corresponds to the existence of a material neutral particle which is described in a manner free from singularities.

The solution of the problem of the motion of neutral particles evidently amounts to the discovery of such solutions of the gravitational equations (written free of denominators), as contain several bridges.

The conception sketched above corresponds, a priori, to the atomistic structure of matter insofar as the " bridge " is by its nature a discrete element. Moreover, we see that the mass constant m of the neutral particles must necessarily be positive, since no solution free of singularities can correspond to the Schwarzschild solution for a negative value of m. Only the examination of the several-bridge-problem, can show whether or not this theoretical method furnishes an explanation of the empirically demonstrated equality of the masses of the particles found in nature, and whether it takes into account the facts which the quantum mechanics has so wonderfully comprehended.

In an analogous manner, it is possible to demonstrate that the combined equations of gravitation and electricity (with appropriate choice of the sign of the electrical member in the gravitational equations) produces a singularity-free bridge-representation of the electric corpuscle. The simplest solution of this kind is that for an electrical particle without gravitational mass.

So long as the important mathematical difficulties concerned with the solution of the several-bridge-problem, are not overcome, nothing can be said concerning the usefulness of the theory from the physicist's point of view. However, it constitutes, as a matter of fact, the first attempt towards the consistent elaboration of a field theory which presents a possibility of explaining the properties of matter. In favor of this attempt one should also add that it is based on the simplest possible relativistic field equations known today.

SUMMARY.

Physics constitutes a logical system of thought which is in a state of evolution, and whose basis cannot be obtained through distillation by any inductive method from the experiences lived through, but which can only be attained by free invention. The justification (truth content) of the system rests in the proof of usefulness of the resulting theorems on the basis of sense experiences, where the relations of the latter to the former can only be comprehended intuitively. Evolution is going on in the direction of increasing simplicity of the logical basis. In order further to approach this goal, we must make up our mind to accept the fact that the logical basis departs more and more from the facts of experience, and that the path of our thought from the fundamental basis to these resulting theorems, which correlate with sense experiences, becomes continually harder and longer.

Our aim has been to sketch, as briefly as possible, the development of the fundamental concepts in their dependence upon the facts of experience and upon the strife towards the goal of internal perfection of the system. Today's state of affairs had to be illuminated by these considerations, as they appear to me. (It is unavoidable that historic schematic representation is of a personal color.)

I try to demonstrate how the concepts of bodily objects, space, subjective and objective time, are connected with one another and with the nature of the experience. In classical mechanics the concepts of space and time become independent. The concept of the bodily object is replaced in the foundations by the concept of the material point, by which means mechanics becomes fundamentally atomistic. Light and elec-

tricity produce insurmountable difficulties when one attempts to make mechanics the basis of all physics. We are thus led to the field theory of electricity, and, later on to the attempt to base physics entirely upon the concept of the field (after an attempted compromise with classical mechanics). This attempt leads to the theory of relativity (evolution of the notion of space and time into that of the continuum with metric structure).

I try to demonstrate, furthermore, why in my opinion the quantum theory does not seem likely to be able to produce a usable foundation for physics: one becomes involved in contradictions if one tries to consider the theoretical quantum description as a *complete* description of the individual physical system or happening.

On the other hand, up to the present time, the field theory is unable to give an explanation of the molecular structure of matter and of quantum phenomena. It is shown, however, that the conviction to the effect that the field theory is unable to give, by its methods, a solution of these problems rests upon prejudice.

VACUUM PUMPS AND PUMP OILS. PART II. A COMPARISON OF OILS.

BY

K. C. D. HICKMAN.*

The preceding paper [1] presents evidence that, in condensation pumps which comply with certain principles of design, the optimum vacuum attainable, using *n*-dibutyl phthalate as a working fluid, is a reproducible characteristic of butyl phthalate. The vacuum is found to be improved by lowering the temperature of the trap, situated between the pump and the gauge, and from the degree of improvement it is concluded that the optimum vacuum is equal to the saturated vapor pressure of the phthalate at the temperature of the trap. Residual gases, other than butyl phthalate, appear to be absent and from this it is concluded that thermal decomposition does not occur to any appreciable extent. The resistance to decomposition makes it probable that other compounds having higher boiling points and lower vapor pressures than the butyl ester can be used to yield higher optimum vacuums. It is the purpose of this paper to present and compare various phlegmatic liquids; to examine whether the optimum vacuums obtainable are equal to the saturated vapor pressures of the pure compounds; to determine the conditions under which each liquid gives the best performance. The subject will be opened with a study of the esters of phthalic acid.

THE HIGHER PHTHALATE ESTERS.

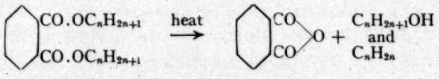
On first consideration the *esters* do not seem particularly suited for pumping fluids. They are compounds which are prepared by the interaction of acids and alcohols with the elimination of water; and by the action of water or water vapor they are again decomposed into constituents which impair a vacuum. The oxygen in their molecules has a

* Communication No. 570 from the Kodak Research Laboratories.

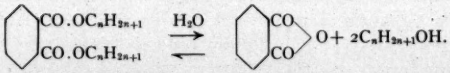
[1] Part I paper.

higher free energy than in carbon dioxide, the most stable oxide of carbon and they can thus be decomposed thermally with the liberation of CO_2 and other products. The phthalic acid from which the phthalates are prepared exhibits a marked tendency to form the anhydride and tarry condensation products. It would hardly be expected that the esters would possess the stability necessary for vacuum service and, of the esters, the phthalates would not appear especially promising. It is found, however, that phthalates containing less than 30 carbon atoms are extremely stable under pumping conditions and are superior to any other liquid organic compounds of similar volatility so far examined by the author. The examination has been by no means exhaustive, being confined to substances common as cellulose plasticizers, and it is realized that many new and more useful pump fluids remain to be discovered.

The reactions to which the aliphatic phthalate esters [2] are sensitive are:



and



The reactions prove relatively unimportant, perhaps because, under the conditions of operation, air and water are necessarily absent. When the esters are boiled in a pump, water is so thoroughly eliminated that a condition of "Bakerian" dryness [3] supervenes in which the decomposition reactions proceed at an extremely low rate. The attack on metals made by the phthalates when heated in air is diminished in a vacuum to such a point that metal pumps may safely be used. N-amyl phthalate has been boiled in brass and steel pumps for months without causing appreciable corrosion. That traces of water are dangerous is easily demonstrated. Occasionally a new

[2] "Pyrolysis of Carbon Compounds," C. D. Hurd, A. C. S. Monograph, Chemical Catalog Monograph, 1929, page 555.

[3] H. B. Baker, *J. C. S.*, 1922, **121**, 568; 1923, **123**, 1223.

glass pump is put into operation and is filled with phthalate without drying the glassware. The wet phthalate boiling in the "steam" vacuum inevitably deposits a crop of phthalic anhydride crystals in the tube leading to the fore pump.

The thermal decomposition according to the second equation, in which water plays no part, is determined by the temperature of operation rather than the complexity of the molecule. Amyl phthalate shows no harmful decomposition at its boiling point under pressures usual in pumps (.1–.4 mm.); whereas, β-phenyl ethyl phthalate evolves gases steadily. When the temperature of the latter is reduced from 215° C. to about 140° C., at which the amyl ester is boiling, the evolution of gas ceases. Unfortunately, pumping ceases also and it must be concluded that the heavier ester is not suitable for use in an ordinary pump.

Organic pump fillings are known to be inferior to mercury in two important, closely related, respects. They evolve large quantities of dissolved air on first applying vacuum and they are damaged if exposed to the atmosphere whilst hot. The esters and the saturated hydrocarbons evolve less gas than compounds containing double bonds. It has not yet been possible to prepare a pumping fluid consisting entirely of saturated hydrocarbons because the latter are solids when sufficiently phlegmatic. The liquid hydrocarbons containing unsaturated bodies require from two to three times as long a period to degas as the esters.

The damage done to pump oils on hot exposure takes place in two stages. First, there is the interaction between the air and oil at the temperature which has been prevailing in the pump, and, then there is the further interaction which ensues as the temperature of the oil rises because boiling has been prevented by the cessation of vacuum.

When used in a three-compartment pump, the phthalate esters are practically insensitive to short or accidental hot exposure. Indeed, the rate of reëstablishment of high vacuum is often greater than the first approach. In this they have proved, in the author's hands, superior to the hydrocarbon oils. On continued exposure at high temperatures, the situation is reversed. The phthalates are resolved into phthalic anhydride and a black, malodorous residue, the

combination possessing no pumping capacity. The hydrocarbon oils after similar treatment, though badly charred, can be made to give moderate service by prolonged boiling in a vacuum.

Resistance to the first stages of hot exposure is the more important attribute because the heat supply can generally be shut off before damage is done. Two minutes' exposure at atmospheric pressure with the heating maintained has been adopted as the standard test in this work. The phthalate esters withstand the treatment unharmed. The resistance to oxidation appears to be conferred by the carboxyl groups. Oxygen atoms linked in other ways and the addition of oxygen atoms to other places in the molecule does not improve the immunity to oxidation but yields a less stable compound. An example may be drawn from Table I where it is recorded that β-methoxy ethyl phthalate is inferior to *n*-hexyl phthalate of similar boiling point.

There are many phthalates of higher boiling point than butyl phthalate which are liquid at room temperature. Isoamyl phthalate was examined [4] in 1929 but was found to possess little advantage over the *n*-butyl ester; and it since appears that the branched chain alcohols are not as stable as the normal compounds.

Recently, a complete series of normal phthalates ranging from the methyl- to the nonyl-ester have been assembled and purified, those not commercially available * having been prepared for the author by Dr. Baxter of this laboratory, to whom grateful thanks and acknowledgments are extended. A number of other esters were added to the collection.

The lighter phthalates are water white liquids. The heavier phthalates, as prepared, were pale yellow, but there were indications that prolonged purification would render them nearly white. The specific gravity varied from 1 to 1.2 but the exact values have not yet been determined. Measurements are also lacking on the latent heat of evaporation and on other useful physical properties. It is hoped to present such data at another time. The phthalates are remarkable

[4] K. C. D. Hickman and C. R. Sanford, *Rev. Sci. Inst.*, 1930, 1, 140.

* Marked with an asterisk in table.

for the great temperature interval between their solidification and boiling points (extrapolated to normal pressure), a property which is essential to their use as vacuum fluids. Unfortunately, phenyl phthalate, which is situated in perhaps the optimum vapor pressure region, is a solid melting at about 74°. Benzyl phthalate, boiling higher, melts lower at 43° C. and will remain supercooled sufficiently readily for use in glass pumps. It was in the hope of securing a compound with even greater liquid range that the β-ethyl phenyl phthalate was prepared. The material proved to be a viscous liquid at room temperature, congealing to a glass in the ice bath.

The boiling points over the range 10 mm.–0.1 mm. were determined in a simple hypsometer to be described elsewhere. The values at 1.0 mm. are recorded in Table I and the vapor pressure curves over the hundredfold range are shown in Fig. 1.

OPTIMUM VACUUM AND THERMAL DECOMPOSITION.

The liquids were now tested for optimum vacuum in the three-compartment pump and by the procedure described in the first part of this paper. The measurements occupied three months in the middle of summer when the temperature seldom fell below 25° C. The trap was immersed in water, held at 25° C., and the vacuums may be considered to correspond fairly accurately to this temperature. The vacuums produced by all the phthalates from amyl- to heptyl- were improved by placing ice round the trap, from which it is concluded that they were equal to the saturated vapor pressures of the compounds at 25° C. A second important conclusion is that all the pure normal aliphatic phthalates up to the heptyl compound, which contains 20 carbon atoms, can be boiled under a pressure of 1 mm. without substantial decomposition. The impairment of vacuum attributed to thermal decomposition has been due to the presence of impurities or to oxidation or hydrolysis, each accentuated by pumps of inadequate design.

The other phthalates mentioned in Table 1 gave vacuums which were insensitive to moderate cooling of the trap. They were evidently evolving products of cracking. Octyl phthalate showed indications of temperature sensitivity, but the

TABLE I.

Name.	Formula.	Approx. B.P. at 1.0 mm.	Experimental Optimum Vacuums (Uncorrected) at Various Trap Temperatures.							
			25° C.		0° C.		− 15° C.		− 78° C.	
			Micro amps.	P mm.	Micro amps.	P mm.	Micro amps.	P mm.	Micro amps.	P mm.
n-amyl phthalate	$COOC_5H_{11}$ $COOC_5H_{11}$	164	1.0	2.5×10^{-6}	.02	5.0×10^{-7}	<.005	1.2×10^{-7}	<.004	
n-hexyl phthalate*	$COOC_6H_{13}$ $COOC_6H_{13}$	179	.12	3.0×10^{-6}	.02	5.0×10^{-7}	<.004	$<10^{-7}$	<.004	
n-heptyl phthalate*	$COOC_7H_{15}$ $COOC_7H_{15}$	195	.01	2.5×10^{-7}	.005	1.2×10^{-7}	<.004	$<10^{-7}$	<.004	
n-octyl phthalate*	$COOC_8H_{17}$ $COOC_8H_{17}$	211	<.004	$<10^{-7}$						
n-nonyl phthalate*	$COOC_9H_{19}$ $COOC_9H_{19}$	223	.008	2.0×10^{-7}					.008	2.0×10^{-7}

* Prepared by Dr. J. G. Baxter of this Laboratory.

TABLE I.—*Continued.*

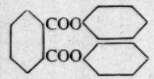
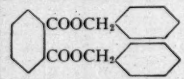
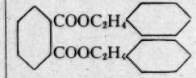
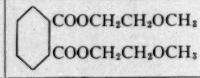
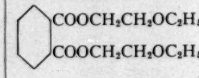
Name.	Formula.	Approx. B.P. at 1.0 mm.	Experimental Optimum Vacuums (Uncorrected) at Various Trap Temperatures.							
			25° C.		0° C.		− 15° C.		− 78° C.	
			Micro amps.	P mm.	Micro amps.	P mm.	Micro amps.	P mm.	Micro amps.	P mm.
Phenyl phthalate		212								
Benzyl phthalate		224	.005	1.2×10^{-7}	.005	1.2×10^{-7}	.005		.005	
β ethyl phenyl phthalate*		237	.12	3.0×10^{-6}	.12		.12		.10	2.5×10^{-6}
β methoxy ethyl phthalate			1.8	4.5×10^{-5}	.13	3.3×10^{-6}	.13		.10	
Ethoxy ethyl phthalate			.95	2.4×10^{-5}	.20	5.0×10^{-6}	.19		.17	4.3×10^{-6}

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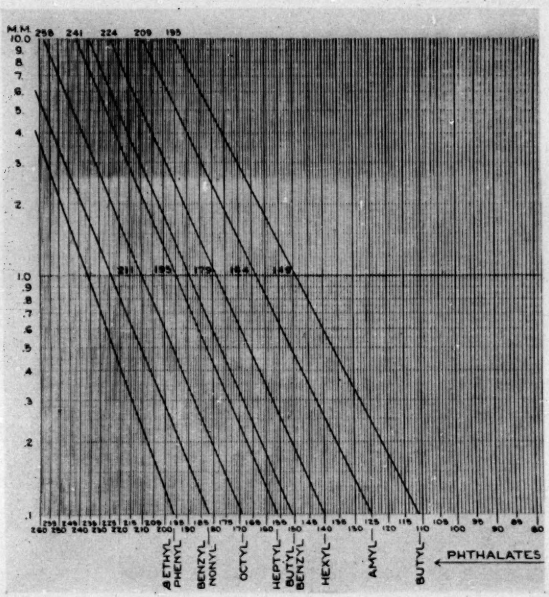
FIG. 1.



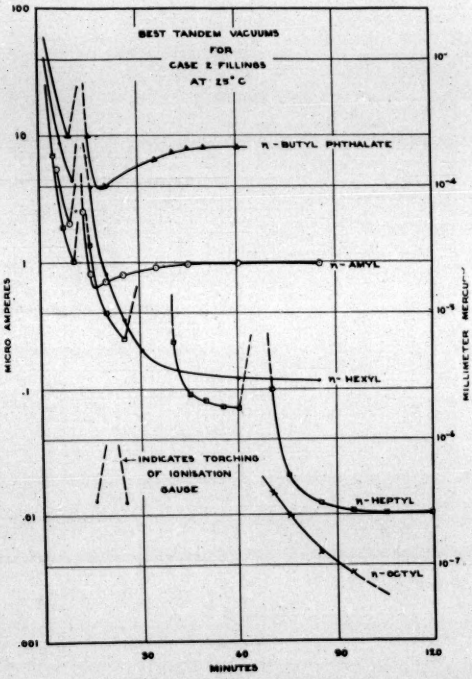
readings were at the limit of measurement by the micro ammeter (.004 micro ampere). *n*-Octyl phthalate produces rapidly a higher vacuum at 25° C. than any other pure compound or commercial pump oil examined.

Since from amyl- to heptyl-phthalate the residual gas in the vacuum was the vapor of the ester, it appeared legitimate to apply a correction to the readings of the ionization gauge. Dushman and Found [5] have shown that the readings are proportional, not to the molecular pressure, but to the total number of extra-nuclear electrons in the gas. Accordingly, the pressure readings should be diminished in the ratio of phthalate electrons to air electrons by a factor ranging from 11 to 15 times.

[5] S. Dushman and C. G. Found, *Phys. Rev.*, 1921, 17, 7.

The optimum vacuums (uncorrected) are recorded in Fig. 2 which also shows the rate of attainment of vacuum for each phthalate. The vertical, broken-line interruptions of the curves indicate periods occupied by torching the ionization gauge. The regular pressure interval between each phthalate

FIG. 2.

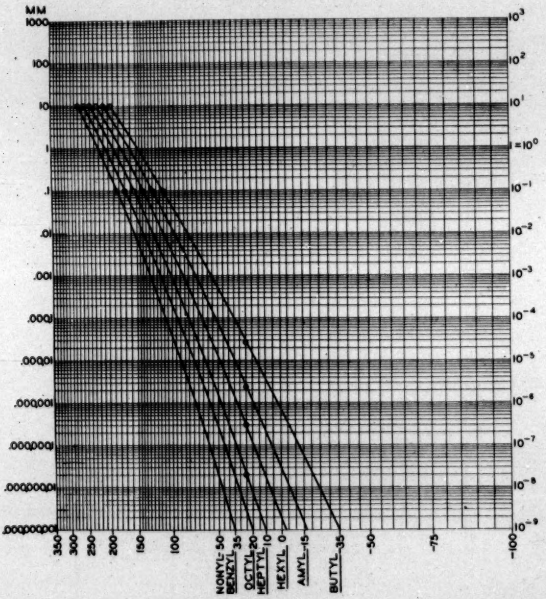


vacuum offers confirmation that the limits are determined by the vapor pressure of the compounds. Applying now the Dushman and Found correction factor, the vapor pressures at 25° C. are seen to be:

Butyl..... 1.9×10^{-5} mm.
Amyl..... 2.1×10^{-6} mm.
Hexyl..... 2.5×10^{-7} mm.
Heptyl..... 1.8×10^{-8} mm.

In Fig. 3, the vapor pressure data from Fig. 1 have been replotted on a chart of wider range and the lines extended to

FIG. 3.



meet the deduced low pressure points. A slight curvature is introduced but it is of the order to be expected for such highly associated liquids. It is believed that the chart presents, to a useful approximation, the vapor pressures of the liquids and the optimum vacuums available.

THE RATE OF ATTAINING HIGH VACUUM.

Reference to Fig. 2 of Part I of this paper will show that the customary large glass storage vessel between the trap and the gauge was absent in the experimental testing apparatus. This was omitted purposely because it was found that the rate of attaining a high vacuum is dependent more on the design of the pump and the nature and mass of the oil filling than on the size of the receiver. An example will make this clear: In a certain experiment, a vacuum of 1.0×10^{-6} mm. was reached in 20 minutes from the time the pump oil commenced to boil. The reduction from 1×10^{-4} mm. to 1×10^{-6} mm. required the last 9 minutes, during which time the micro ammeter needle of the ionization gauge passed over 99 per cent. of its range. A little air was then admitted which increased the pressure until the needle swung off the scale. The needle returned to 1.2×10^{-6} mm. in 4 seconds after shutting off the leak. The rate of attainment of vacuum is thus the rate of conditioning of the pump oil and is a separate phenomenon from the speed of the pump at constant pressure, which is dependent on the size and shapes of the jets, the admittance of the lead tubes, and other factors which have been thoroughly investigated by various workers. The rate of conditioning is of great practical importance. It is the factor which determines the period necessary to secure a high vacuum in an assembly which has previously been at atmospheric pressure. Preliminary experiments have shown that conditioning is not as rapid in a simple pump as in one of three compartments and it is in a pump of this type, as described in Part I, that the testing of the oils has been carried out. For the three-compartment pump, there may be distinguished three varieties of conditioning:

Case 1: The filling is new.

Case 2: The filling has been used and the vacuum has been relieved after cooling.

Case 3: The filling has been used and the vacuum has been broken, perhaps accidentally, whilst the pump is hot or in use.

The rate of conditioning is almost instantaneous with a mercury condensation pump. With oil pumps, it is more rapid with a saturated compound having little affinity for air than for an unsaturated compound, and in this respect,

the phthalate esters are particularly useful. The conditioning is slowest in Case 1 with a new filling which requires time for the constituents to travel to the appropriate parts of the pump. Case 2 is the one most often encountered, but it is not the most important. The spoiling of pumping capacity by hot exposure disqualifies oil vapor pumps from many industrial operations. A multi-compartment pump, while not remedying this Case 2 spoilage completely, can minimize it because the products of oxidation are likely to be more or less volatile than the parent liquid. Either they will be rejected by the fractionating column or they will pass into the sink for non-volatiles, to decompose at leisure into the intermediate vacuum. Thus, the Apiezon oils, which ordinarily turn a deep and permanent brown on hot exposure to air, regain a lemon yellow color in a short time after again boiling under vacuum in a three-compartment pump. The black, charred, constituents become concentrated in the sink. The phthalates, which are insensitive to moderate hot oxygen exposure, often yield a vacuum more rapidly under Case 3 than Case 1 or 2 conditions. The conditioning times for the more important liquids are given in Figs. 4 and 5 and Table 2.

At the outset of these experiments, no standard of performance was available and it was considered desirable and proper to use as relative standards the two Apiezon oils (A and B) which have reached a well-merited preëminence in vacuum technology. In appraising the results which follow, it should be remembered that the oils prepared in this Laboratory were used within a few weeks of synthesis, whereas the Apiezon oils were obtained from England, and one sample was at least two years old. They had, therefore, opportunity for acquiring water and products of oxidation which were known to be absent from the freshly prepared compounds. It was considered unwise to redistill the Apiezon oils before use, since this might have contributed unfavorably to their thermal history. The conclusions in this paper should be accepted with the greatest caution and considered valid only when confirmed in laboratories more generally concerned with high vacuum technique than the author's.

The rate of conditioning of the three phthalates, butyl, amyl, and hexyl, is such that their optimum vacuums are

FIG. 4.

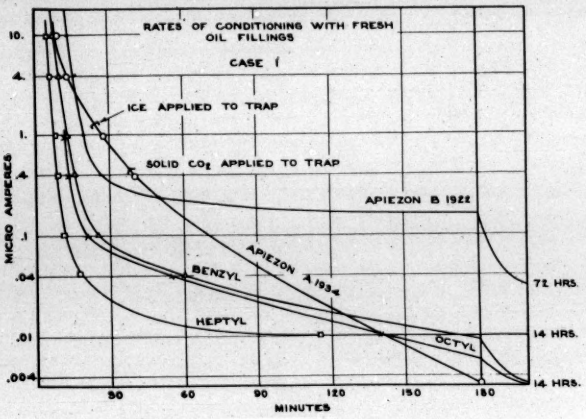

RATES OF CONDITIONING WITH FRESH OIL FILLINGS
CASE I
ICE APPLIED TO TRAP
SOLID CO_2 APPLIED TO TRAP
APIEZON B 1922
APIEZON A 1934
BENZYL
HEPTYL
OCTYL
72 HRS.
14 HRS.
14 HRS.
MICRO AMPERES
10.
4.
1.
.4
.1
.04
.01
.004
30
60
90
120
150
180
MINUTES

FIG. 5.

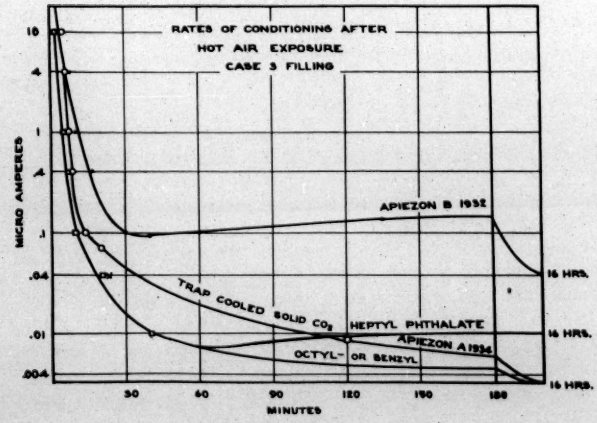

RATES OF CONDITIONING AFTER HOT AIR EXPOSURE
CASE 3 FILLING
APIEZON B 1932
TRAP COOLED SOLID CO_2
HEPTYL PHTHALATE
APIEZON A 1934
OCTYL- OR BENZYL
16 HRS.
16 HRS.
16 HRS.
MICRO AMPERES
10
4
1
.4
.1
.04
.01
.004
30
60
90
120
150
180
MINUTES

TABLE 2.

25° C. P mm. Hg	Case 1—Conditioning Time * of Fresh Filling								Case 3—Conditioning Time * after Hot Exposure									
	10^{-3}		10^{-4}		10^{-5}		10^{-6}		10^{-7}		10^{-3}		10^{-4}		10^{-5}		10^{-6}	
Micro amperes	40	10	4	1	4	.1	.04	.01	.004	100	40	10	4	1	.4	.1	.04	.01
n-butyl phthalate													4	9	[10	15	20	60
n-amyl phthalate																		
n-hexyl phthalate	2¼	6	10	19	23	100	[	160										
n-heptyl phthalate	3¼	4½	6	8	9	10	16	115				1¼		5	7	9	20	40
n-octyl phthalate	3½	6	9	11	13	20	55	140	14 hrs.				4	8	13	16	21	200 16 hrs.
Benzyl phthalate	4	7	9	12	14	24	60		14 hrs.		1½	3	4	6	7	11	20	16 hrs.
Ethyl phenyl phthalate	6	9	13	16	35		48 hrs.				2¼	4	5	7	9	15	60	
Methoxy ethyl phthalate	1½	2½	4	[7	13	60						4	5	7	[14	25	60	
Ethoxy ethyl phthalate	5	8	10	14	[30													
Apiezon A 1934	1	9	12	[27	40	<			180	¾	1¾	15	20	44	<		110	100 24 hrs.
Apiezon A 1932					120	[	4 hrs.					<4½	6	7	8	13		
Apiezon B 1934	10	13	16	19	26		72 hrs.				1	3½	7	9	[16	30	8 hrs.	

* The figures refer to time in minutes after the pump fluids start to boil. [, ice is applied to the trap beyond this point. <, solid CO_2 is applied to the trap beyond this point.

reached in 10 to 30 minutes. Only with the more phlegmatic members are the conditioning times pronounced. The liquids shown on the chart for Case 1 fillings are, therefore, heptyl, octyl, and benzyl phthalates and the Apiezon oils. The heavier Apiezon B reached a pressure limit registering .2 micro amperes on the ionization gauge after three hours and the optimum vacuum was not attained until after 72. Neither of these vacuums was sensitive to moderate lowering of trap temperature and it must be concluded that the vacuum was being impaired by products of decomposition. Heptyl phthalate, on the other hand, gives relatively rapid conditioning, the optimum vacuum being reached in two hours and the vacuum is sensitive to trap temperature. Benzyl and octyl phthalates give higher vacuums but require longer for this first conditioning. The curves shown do not represent the characteristics of the material as pure chemical compounds, but are a history of the rate of establishment of vacuum by the particular samples under test. The optimum vacuum for Apiezon A (1934) corresponded with three micro amperes and the curve would thus find no place in Fig. 4. Instead, a curve is included from an experiment in which first ice and then solid carbon dioxide was applied to the trap. The early conditioning is relatively slow but the pressure attained in three hours was as low as that given by benzyl and octyl phthalate at 25° C. after 14 hours. In a parallel experiment, it was determined that amyl and hexyl phthalates trapped with solid CO_2 will yield a vacuum below the limits of measurement in three hours.

The degree of maltreatment, adopted as standard in the experiments on hot exposure, was the admission of air at atmospheric pressure for exactly two minutes to the pump, previously boiling under vacuum, the heating current being maintained during the exposure. It was found that the temperature would rise 50° to 100° C. during the interval and that boiling would begin again at a pressure of about 2 mm. as the vacuum was reëstablished. Butyl, amyl, and hexyl phthalates showed no indications whatsoever of harm by this treatment, the attainment of optimum vacuum generally being quicker than under Case 1 conditions. Apiezon oil A (1932), when used with a refrigerated trap, gave an extremely

rapid reëstablishment of moderate vacuum, but the optimum pressure was not reached until after 16 hours. The oil was, however, much less sensitive to exposure than had been anticipated. Apiezon B reconditioned rapidly to .1 micro amperes after which the pressure rose very slightly for three hours to fall again almost to the optimum vacuum in the next 16 hours. The three heavy phthalates gave vacuums corresponding to .1 micro amperes in 15 minutes and .01 micro amperes in 40 minutes. The optimum vacuums were approached in three hours and completely established in 16.

The curves for nonyl- and ethyl-phenyl phthalates are not included in the figures because the optimum vacuums are inferior to those of the heptyl and octyl phthalates. The heavier esters boil at too high a temperature to withstand decomposition and they are correspondingly more sensitive to hot exposure. Phenyl phthalate probably possesses the most suitable boiling point for pumping service, but the material is ordinarily a solid and the expansion during remelting is dangerous to glass pumps. The compound can be used in metal pumps if the solid which deposits on the intake and exit pipes is not considered objectionable.

THE CHOICE OF PUMP OIL.

The highest (unmeasurable) vacuums recorded in this work were secured by using Apiezon A, amyl phthalate, or hexyl phthalate with a trap cooled by dry ice. When a mixture of ice and salt was used to replace the solid carbon dioxide, an equally high vacuum was obtained with hexyl phthalate. The vacuums given by Apiezon A and amyl phthalate trapped with ice and salt were nearly as good.

The highest vacuum obtained without a cooled trap at a room temperature of 25° was secured with octyl phthalate. Only slightly inferior were heptyl and benzyl phthalates. Heptyl phthalate proved to be the fastest in operation and most resistant to maltreatment. Either heptyl or benzyl phthalates employed in a three-compartment pump or other pump with fractionating features provide a ready source of vacuum of the order 10^{-6} to 10^{-7} without the use of an absorbent or refrigerated trap.

Equally low pressures can be obtained with hexyl phtha-

late and a trap cooled with ice, or with amyl phthalate and ice and salt.

For routine work with molecular distillations on a laboratory or industrial scale *n*-amyl phthalate has proved serviceable. It is readily prepared and yields a vacuum with a mean-free path up to a meter without a cooled trap and is insensitive to moderate hot exposure.

Butyl phthalate, having served to introduce its less volatile homologues, must seek retirement as a filling for manometers and for Booster pumps working against relatively high pressures. Its place should be taken by the *n*-amyl ester in those cases where it has previously been recommended. Nevertheless, it remains a reliable filling where a refrigerated trap is not objectionable.

The pump fluids mentioned above can all yield vacuums approaching their vapor pressure at room temperatures. The less stable liquids mentioned in Table 1 will yield pressures below 10^{-5} mm. in a fractionation pump and are satisfactory for many purposes. None of the phthalates give good service in unsuitable pumps.

APPENDIX.

Apart from the desirability of using a pump having fractionating features, certain other elements of design require modifying for use with the heavier phthalates.

Boiler and Heater Design.

Vacuums derived with all the more phlegmatic fluids, including benzyl phthalate and Apiezon B (see Table 3) are insensitive to lowering the trap temperature, showing that thermal decomposition is calling a halt to further reduction in pressure. The quantity of decomposition product liberated is proportional to the quantity of pump filling; hence, the emptier the pump, the better the vacuum. On the other hand, if too shallow a pool is present, the liquid may assume a spheroidal state, a vapor layer, where accelerated decomposition occurs, isolating the liquid from the very hot wall. The optimum depth of liquid appears to be 3 mm. to 5 mm. The heat is usually applied to the under side of the boiler, but it is always dissipated at the evaporating surface of the

liquid, and between the two regions there may be a large thermal gradient. It is important, therefore, in laboratory sized pumps to employ a much larger boiler area than is provided by a mere downward extension of the pump body. Liquids boiling at atmospheric pressure become agitated with vapor bubbles; liquids evaporating in diffusion pumps rely on convection and surface tension "twitches" to convey hot liquid to the surface. The larger the area available, the less the superheating of the liquid and the less the decomposition. The inclusion of nickel or silver wire gauze spiral as suggested by Becker and Jaycox [6] is excellent to convey heat between the two surfaces, but the device should not be used in a deep cylindrical boiler. In fact, a cylindrical boiler of the kind shown in Fig. 6(a) is most unsuitable for oil fillings.

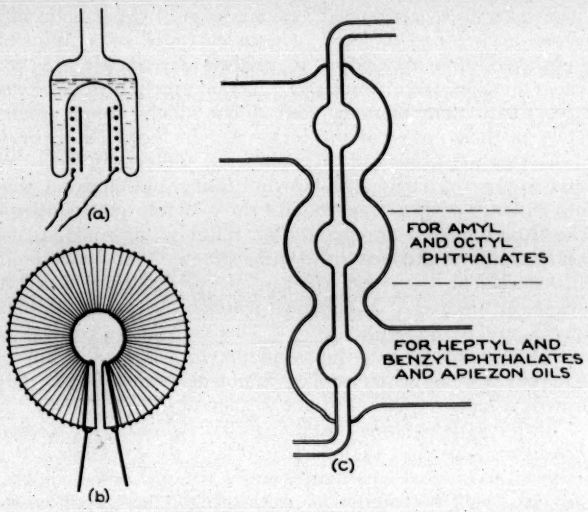
The electrical heating element should be small and light with a minimum heat capacity so that on accidental hot exposure, the temperature will fall as soon as the current is cut off. Nichrome wire windings on split mica circles are very effective (Fig. 6). Excellent heating is secured when a spiral electric winding is immersed in the pump fluid.

Jet Design.

The average thermal velocity of molecules leaving the male jet of a condensation pump is 10 to 20 times greater than the bulk forward velocity of the gas stream. Consequently, as the stream leaves the mouth of the jet, it tends to burst sideways and much of the vapor passes harmfully in the reverse direction towards the high vacuum intake. Recognizing this, Crawford [7] designed a flared jet in which the molecules separating sideways have an increased chance of being reflected in a forward direction. Crawford's design was an inverted one in its preferred form. Unfortunately, the ratio of mass to projective area is less favorable with oils than with mercury and oil molecules cannot be given sufficient kinetic energy to persist against as high a backing pressure as mercury. Furthermore, the oil molecules are extremely easily condensed by cold surfaces. Whereas, a mercury molecule

[6] J. A. Becker and E. K. Jaycox, *Rev. Sci. Inst.*, 1931, **2**, 773.
[7] W. W. Crawford, *Phys. Rev.*, 1917, **10**, 557.

FIG. 6.



may strike a glancing blow at a cooled surface and rebound more than once before condensing; oil molecules generally condense in the first strike and reëvaporate considerably later. Reflected molecules retain a portion of their forward energy after collision, whereas reëvaporated molecules are emitted at random. It is manifestly desirable to cool the receiving jet of a condensation pump to prevent reëvaporation of the condensed oil in the reverse direction. On the other hand, complete cooling cannot be employed with the phlegmatic phthalate esters without so robbing the forward-traveling gas of its velocity that pump efficiency is impaired. It is necessary, therefore, to adjust the temperature of the cooling jacket to suit the pump oil. Butyl and amyl phthalate will tolerate cold water, hexyl and heptyl phthalates operate best with a warm water jacket, and benzyl phthalate gives an optimum performance with nearly boiling water or with air cooling.

Trap Design.

Even with well-designed jets, a stream of vapor molecules passes back into the high vacuum entrance to a diffusion pump and it is imperative to provide a baffled trap even where a cooled trap is omitted. Organic molecules condense more easily than mercury, but differ widely among themselves in their power of survival. Amyl-, hexyl-, and octyl-phthalates wet glass and are condensed sufficiently with two interruptions of path. Heptyl- and benzyl-phthalates gather into globules on glass and require three or four interruptions. The Apiezon oils appear to be roughly intermediate in condensibility. An exit tube with three or four bends is an effective shield, but it has a low admittance and is difficult to jacket with water. A compact water-cooled trap of high admittance is shown in Fig. 6. This trap or its equivalent should be used wherever high vacuum with large admittance is required. The water cooling is not necessary, but it may provide a higher vacuum in hot weather.

With regard to traps containing charcoal or drying agents, the author feels that their inclusion begs the question. If a pump oil has a real vapor pressure of 10^{-7} at room temperature, it should be capable of exhausting a leakproof vessel to this degree of vacuum without adventitious aid. Water vapor should be eliminated as readily as other gases. That the inclusion of P_2O_5 and charcoal has been a prevalent practice is an admission that the pump fillings are reacting with oxygen and evolving traces of decomposition product. At the conclusion of the study detailed in these papers, it is felt that suitable fillings used in fractionating pumps should definitely remove the need for traps filled with chemical or porous absorbents.

A FURTHER STUDY OF OSCILLATORY CIRCUITS HAVING PERIODICALLY VARYING PARAMETERS.

BY

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ABSTRACT.

This paper presents the results of an extensive investigation of a circuit containing resistance, inductance, and a periodically varying capacitance. The problem is important in connection with: frequency modulation in radio transmitters; warble tone generation in electro-acoustics; certain systems in mechanical and acoustical engineering where the elastance or mass parameter varies periodically; and, possible other uses of the circuit in both communications and power engineering as its properties become better understood. The oscillations of this circuit may be classified as: (1) *natural;* (2) *spontaneous;* and (3) *forced.* Type (1) is the usual exponentially decaying oscillation; type (2) is an oscillation induced by the periodic capacitance variation and has either an exponentially increasing amplitude or a constant one; type (3) is the steady state current produced by an impressed a.-c. voltage. All three types of oscillations were thoroughly explored with the result that several new phenomena were observed and *quantitative* data were obtained for a wide range of circuit conditions. In some instances, the method used was mathematical. One result secured in this way was the development of a method for calculating the steady state current forced by an a.-c. voltage. Machine solutions were made with the M. I. T. Differential Analyzer and a series of curves of the instantaneous values of charge and current were obtained for representative circuit adjustments. An experimental investigation of the behavior of actual circuits was also made. Striking results were: the generation of electrical power in excess of one kilowatt by means of spontaneous oscillations; the discovery of the properties of *quasi-resonance* and of *phase sensitivity* in varying parameter circuits. Several applications of these circuit properties to electrical machinery are discussed.

INTRODUCTION.

Recent research in the field of circuits with periodically varying parameters * has resulted in an increased knowledge of these circuits and in effective methods of analyzing their

* A periodically varying parameter is a parameter the value of which varies periodically with the time *alone*. Confusion of a varying parameter with a non-linear parameter is not uncommon, but should be avoided. The value of a non-linear parameter is a function of the current through or voltage across it, and this value *does not vary independently with time*.

performance.[1, 2, 3, 4] This is particularly true of the series circuit composed of a positive resistance, an inductance, and a periodically varying capacitance that forms the subject of this paper. In addition to its direct application to frequency modulation and warble tone generation, this circuit, by virtue of an identical mathematical formulation, has indirect bearing on certain mechanical, acoustical, and other oscillatory systems [5] where the elastance or mass parameters undergo a periodic variation with the time and where the motive force is proportional to the displacement. It is also to be expected that, as the unusual properties of this circuit become better understood, many other useful applications of it in communications and power engineering will be found.

Some of these properties have already been discovered. The history of the circuit begins in 1887 with a paper by Lord Rayleigh,[6] and significant progress has since been made, especially during the last few years. Nevertheless, this work has been largely qualitative in nature and has been limited to a study of the free oscillations of the circuit. The present investigation goes beyond previous ones, in the following respects: (1) the forced oscillations produced by an impressed sinusoidal voltage are explored and a method is developed for calculating the steady state current; (2) a series of *quantitative* curves of the current wave form and amplitude are obtained for natural, spontaneous, and forced conditions by employing mathematical, experimental, and machine methods of analysis; (3) some new experimental results are secured. In the course of the investigation, two new circuit phenomena have been disclosed; they have been termed "quasi-resonance" and

[1] Bibliographies are found in references 2 and 5. Since completing this work, a paper by Arthur Erdélyi, *Annalen der Physik*, **19** (6), p. 585 (1934), has appeared; his treatment is restricted to relatively slow variations.

[2] W. L. Barrow, *Proc. I. R. E.*, **21** (8), p. 1182 (1933).

[3] W. L. Barrow, *Proc. I. R. E.*, **22** (2), p. 201 (1934).

[4] F. W. Baumann and D. B. Smith, Master's Thesis in the Electrical Engineering Department, June, 1934, Massachusetts Institute of Technology.

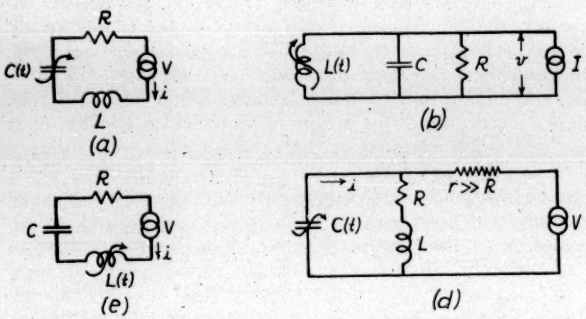
[5] M. J. O. Strutt, "Lamésche-, Mathieusche- und verwandte Funktionen in Physik und Technik," Ergebnisse der Mathematik, Band I, Hirschwaldsche Buchhandlung, Berlin (1932).

[6] Lord Rayleigh, Scientific Papers, Vol. 3, p. 1 (1887), and Vol. 2, p. 88 (1883).

"phase sensitivity" respectively and are characteristic of periodically varying circuits containing both inductance and capacitance.

The circuit involved in most of these investigations is that of Fig. 1*a*. The type of voltage source V and the capacitance

FIG. 1.



The four quasi resonant circuits discussed in this paper. The elements having a superposed sinusoidal arrow vary periodically with the time. The particular function $C(t)$ or $L(t)$ is given in the text, as is the nature of the voltage source V or current source I.

variation $C(t)$ will be specified in each subsequent section. Although a varying inductor was available for experimental tests, no entirely suitable condenser could be obtained; consequently, with the time at our disposal, it was necessary to use the dual circuit of Fig. 1*b* in some of the experiments. Its behavior will be the same as that of 1*a*, provided: V and I and i and v are interchanged; $r \ll R$; and $\frac{r}{L(t)} \ll \frac{1}{R}$, where r is the resistance of the varying inductor.

For the sake of clarity in the presentation, the material to follow has been divided into two parts, viz.: Part I, dealing with the behavior of the circuit in the absence of any source of voltage; and, Part II, treating the steady state response to an impressed sinusoidal voltage. In each part, a further subdivision into sections containing the mathematical analysis, the machine solutions, and the experimental results has been made.

PART I.

The Natural and the Spontaneous Oscillations of the Dissipative Circuit.

Preliminary Discussion.

The aspect of the general problem that is first considered in this paper is the way in which the charge and the current vary with the time, after closing the circuit; the condenser is assumed to be initially charged. Were the parameters all constant, the charge and current would be the exponentially damped oscillation or aperiodic discharge that constitutes the well-known natural oscillation of a resonant circuit. However, the charge and the current behave in a very different manner when one of the parameters varies periodically, as it does here. The diagram of the varying parameter circuit is shown in Fig. 1*a*, where the resistance R and the inductance L are constant, the capacitance varies periodically with time in a manner defined by $C(t) = C_0/(1 + h \cos \alpha t)$, and it may be considered that a steady e.m.f. V is short circuited to initiate the transient. The periodic capacitance variation may be produced by rotating a variable condenser, by causing an acoustic wave to impinge on a condenser microphone, or by any other suitable means.

Mathematical Analysis.

The differential equation for the charge will now be set up and its solution obtained; the behavior of the circuit will then be interpreted from this solution. Denoting the charge by Q, the mean angular frequency $(LC_0)^{-1/2}$ about which the variation takes place by ω_0, the degree of frequency variation by $h(0 \leqq h \leqq 1)$, the frequency with which the variation takes place by $\alpha/2\pi$, and the time by t, the differential equation for the circuit is

$$\frac{d^2Q}{dt^2} + \frac{R}{L}\frac{dQ}{dt} + [\omega_0^2 + \omega_0^2 h \cos \alpha t]Q = 0. \qquad (1)$$

The damping term may be removed by the substitution $Q = q \cdot e^{-Rt/2L}$, where $q = q(t)$ satisfies the differential equation:

$$\frac{d^2q}{dt^2} + \left[\omega_0^2 - \left(\frac{R}{2L}\right)^2 + \omega_0^2 h \cos \alpha t\right]q = 0. \qquad (2)$$

Making the convenient change of variable $\alpha t = \tau$, this equation becomes:

$$\frac{d^2q}{d\tau^2} + [W' + A \cos \tau]q = 0, \qquad (3)$$

where:

$$W' = \left(\frac{\omega_0}{\alpha}\right)^2 - \left(\frac{R}{2L\alpha}\right)^2, \qquad A = \left(\frac{\omega_0}{\alpha}\right)^2 h.$$

Equation (3) has the form of the Mathieu equation and according to Floquet's theory the solution is:

$$q = c_1 e^{-i\mu\tau}\phi(-\tau) + c_2 e^{+i\mu\tau}\phi(\tau), \qquad (4)$$

where $\phi(\tau)$ is a periodic function, $\mu = a + ib$ is, in general, a complex number called the characteristic exponent, c_1 and c_2 are constants determined by the initial conditions, and $i = \sqrt{-1}$. The Mathieu equation often occurs in physics and engineering,[5] usually in connection with a boundary value problem. The solutions in this kind of problem are required to be periodic. The parameter A is restricted to one or more constant values and W' is so determined that the solutions are periodic. However, in the electrical case considered here, the values of W' and A are *both* fixed by the circuit adjustment, and the solution, which is *not* required to be periodic, is sought. Whether it is periodic or not depends entirely on the values of the circuit parameters. This fact, together with the possibility of a relatively large variation of elastance or mass and a considerable interest in the forced oscillations produced by a simple harmonic e.m.f. (or motive force), characterize the present problem and differentiate it from the usual one involving the Mathieu equation. The real part of μ affects the frequency of the oscillations and hence $e^{\pm i a \tau}$ may be combined with $\phi(\pm \tau)$ to give $\phi_1(\tau)$ and $\phi_2(\tau)$. The general solution for the original equation (1) is therefore:

$$Q = c_1 e^{[ba-(R/2L)]t}\phi_1(\alpha t) + c_2 e^{-[ba+(R/2L)]t}\phi_2(\alpha t). \qquad (5)$$

Because of its relatively large negative exponent, the second term approaches zero much more quickly than the first.

Consequently, the main interest attaches to the first term, which may be conveniently written as:

$$Q_1 = e^{\delta t}\phi_1(\alpha t), \qquad \delta = b\alpha - \frac{R}{2L}. \tag{6}$$

Here, δ is the damping coefficient, and $\phi_1(\alpha t)$ is a sum of simple harmonic oscillations.

It is clear that δ may be either positive, negative or zero, depending on the values of b, α, R, and L. Two distinct types of oscillation * are therefore possible, as follows:

1. *Natural Oscillation*, occurring when δ is negative and dying away with the time. This type corresponds to the natural oscillations of constant parameter circuits.
2. *Spontaneous Oscillation*, occurring when δ is zero or positive. In this case, energy is transferred from the mechanical system used for varying the capacitance (or inductance) to the electrical circuit, and the current may remain constant or increase with time. There is no corresponding type of oscillation in constant parameter circuits.

The natural oscillation will not be discussed, as we are here interested primarily in the spontaneous oscillations. When $b\alpha > R/2L$, the damping is positive, and the current amplitude increases exponentially with time. This "unstable" condition is physically impossible except for a short time, and one or more of the circuit parameters must change until a "stable"condition is reached. This follows, because, a fixed positive damping implies a mechanical system capable of supplying an ever increasing power to vary the capacitance, which system is obviously unrealizable. Thus, an initial adjustment to an unstable condition will be rapidly modified,

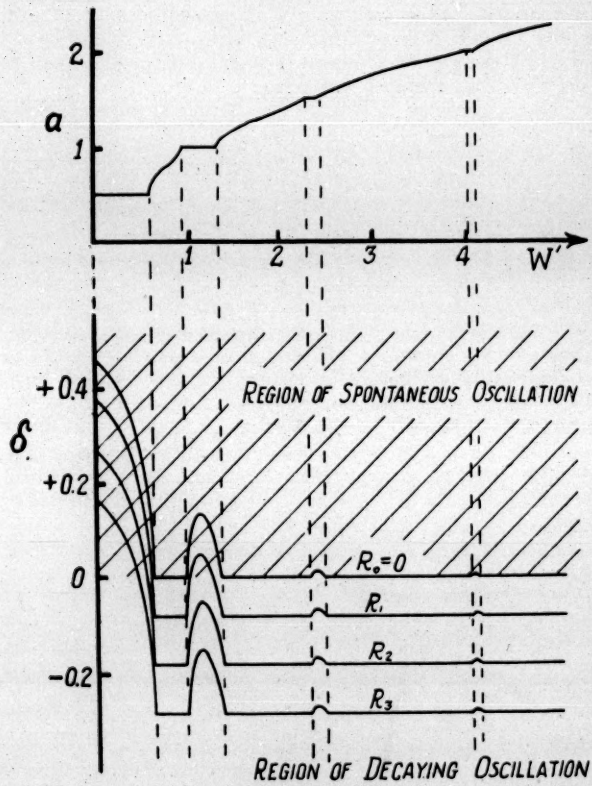
* The term "spontaneous oscillation" is herein applied to an undamped or exponentially increasing oscillation whose existence is due entirely to the periodic variation of the capacitance (or inductance). It is thought that the term "unstable oscillation," carried over from mathematics, is misleading and should be applied only to the transient period during which the current is building up.

The natural oscillations as defined here may have a negative damping coefficient that is smaller than $R/2L$. Nevertheless, this classification is desired, because it separates clearly the two physically different types of behavior.

in any actual circuit, so that a stable spontaneous oscillation ($b\alpha = R/2L$) takes place. When so adjusted, the circuit may be considered as a distinct type of alternating current generator capable of converting mechanical work into electrical power; a description of the experimental accomplishment of this generation will be given later.

To complete the details of the solution, it is necessary to evaluate a, δ and $\phi(t)$ as functions of the circuit parameters. This is, in general, a most difficult task, as the methods of dealing with the Mathieu equation are both laborious and unsatisfactory. It is possible to show the principle characteristics of the circuit, however, by giving a few curves of a, δ, and of the adjustments for spontaneous oscillations. Curves of a and δ as functions of $W = (\omega_0/\alpha)^2$ are reproduced in Fig. 2, for h = a constant and for several different values of R. The effect of a is to change the frequency of the oscillations. As apparent from the figure, one curve is sufficient to portray the dependency of a on the circuit adjustment when A = a constant. The value of the damping coefficient δ determines whether the oscillations decay, build-up, or remain constant; thus, spontaneous conditions occur when $\delta \geqq 0$, unstable spontaneous ones when $\delta > 0$ and decaying natural oscillations for $\delta < 0$. It may be seen from Fig. 2 that δ may assume relatively large values only when $0 < W' < \frac{1}{2}$, and consequently it is only within this range of W' that strong unstable oscillations occur. As W' is increased in value beyond $\frac{1}{2}$, other ranges of values are found for which instability may occur, but, because of the greatly reduced values of δ in these ranges, this tendency becomes smaller and smaller as successive unstable regions are reached. Increasing R also reduces the extent and magnitude of the spontaneous oscillation conditions. When $b\alpha < R/2L$, i.e. when δ is positive, natural oscillations only are possible, no matter what the adjustment, because the mechanical energy transferred to electrical energy is then insufficient to supply the dissipation. These curves make clear the observed fact, that, in practical systems, spontaneous oscillations may usually be produced in the first region, seldom in the second, and almost never in the others. Oscillations of a true undamped character may occur under two circumstances, namely: (1)

FIG. 2.



Curves showing a and the damping coefficient δ as a function of the circuit parameters; $W' = (\omega_0/\alpha)^2 - (R/2L\alpha)^2$. As the resistance R increases the ranges of W' that give spontaneous oscillations (δ positive) become less, finally vanishing altogether.

$R = 0$, $h = 0$; or (2) $R > 0$, $b\alpha = R/2L$. The first case represents a neutralization of the circuit resistance, such as might be effected with a feed-back vacuum tube arrangement. The second case represents an energy transfer, of sufficient magnitude to support the circuit losses, from the mechanical system to the electrical circuit.

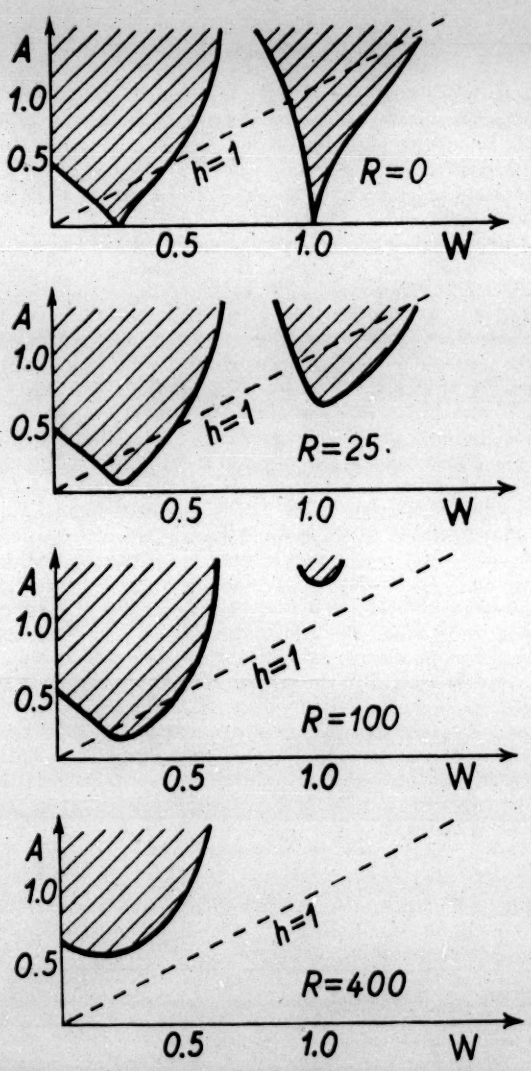
One helpful representation of the behavior of the circuit is that in which the occurrence of spontaneous or of natural oscillation is shown graphically as a function of the quantities W and A. As shown in Fig. 3, the entire W, A plane is thus divided into regions of unstable or of decaying oscillation by means of contours which themselves define the circuit adjustments for undamped spontaneous oscillation. As R is made larger, the areas of instability—the shaded areas of the figure—become smaller and smaller.* Only that portion of the plane between the abscissa and the 45° dotted line corresponds to physically realizable circuit conditions; because h cannot be greater than unity. It is seen from the figure that the first region is the only one allowing unstable oscillation when $R = 100$ ohms, and that spontaneous oscillation is utterly impossible when $R = 400$ ohms. These figures again demonstrate the fact that a spontaneous type of oscillation cannot occur when the resistance exceeds a certain value. They also show why, in a practical case, where the dissipation will have at least a moderate value, the production of such spontaneous oscillations requires a critical and accurate adjustment of the several circuit parameters. In all instances, the ease of production and the magnitude of these oscillations increases with the degree of variation h, and a value near unity will be required to make most practical systems act as spontaneous generators.

Differential Analyzer Solutions.

The calculation of curves of charge and current for a series of circuit conditions was too laborious to be attempted,

* Very few of the complex values of μ are known. The curves of Figs. 2 and 3 were computed from previous results (reference 1) and from the results of studies made with the Differential Analyzer. No pretense of great accuracy is made, but the orders of magnitude are certainly correct.

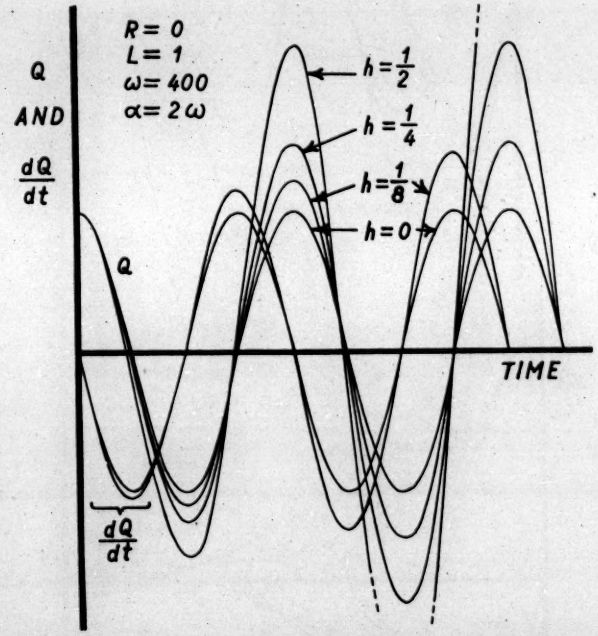
FIG. 3.



Regions of natural (unshaded) and of spontaneous (shaded) oscillation in the $W = (\omega s/\alpha)^2$, $A = h.W$ plane. The effect of increasing the circuit resistance R is brought out by the four diagrams.

and therefore a machine solution was made with the M. I. T. Differential Analyzer.[7, 4] Such curves not only allowed the formation of a broader view of the phenomena but also supplied several numerical values of μ used in constructing the curves of Fig. 3. The operation of the Differential Analyzer in solving equation (1), its set-up, etc. has already been described;[3] this discussion will therefore be confined to the results. A few of the actual machine solutions are reproduced in Fig. 4. These curves show several cycles of

FIG. 4a.



[7] This effective machine for solving differential equations has been adequately described elsewhere. See, for example, V. Bush, JOURNAL OF THE FRANKLIN INSTITUTE, 212 (4), Oct. 1931.

FIG. 4b.

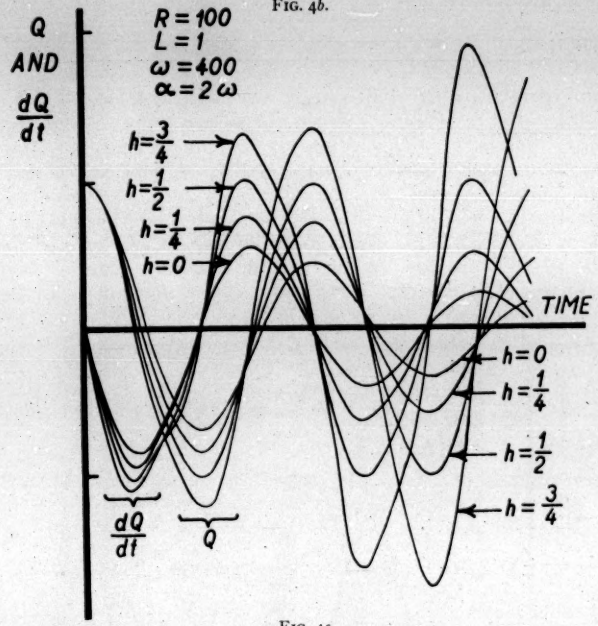

Q
AND
dQ/dt
R = 100
L = 1
ω = 400
α = 2ω
h = 3/4
h = 1/2
h = 1/4
h = 0
TIME
h = 0
h = 1/4
h = 1/2
h = 3/4
dQ/dt
Q

FIG. 4c.

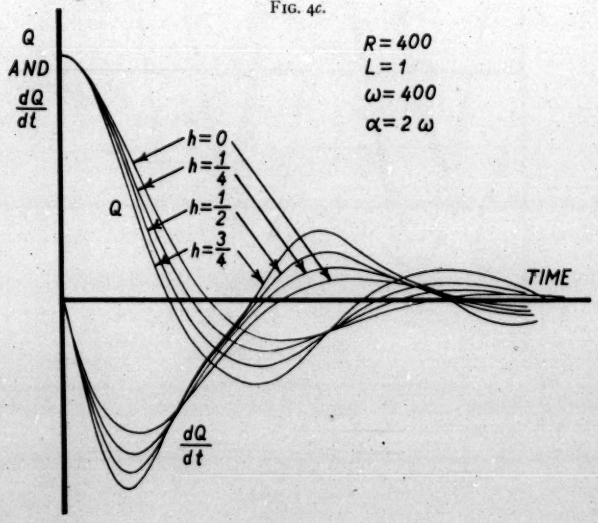

Q
AND
dQ/dt
R = 400
L = 1
ω = 400
α = 2ω
h = 0
h = 1/4
h = 1/2
h = 3/4
Q
TIME
dQ/dt

FIG. 4d.

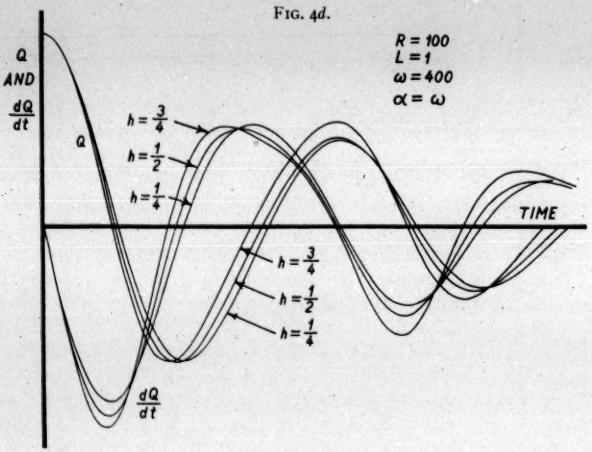
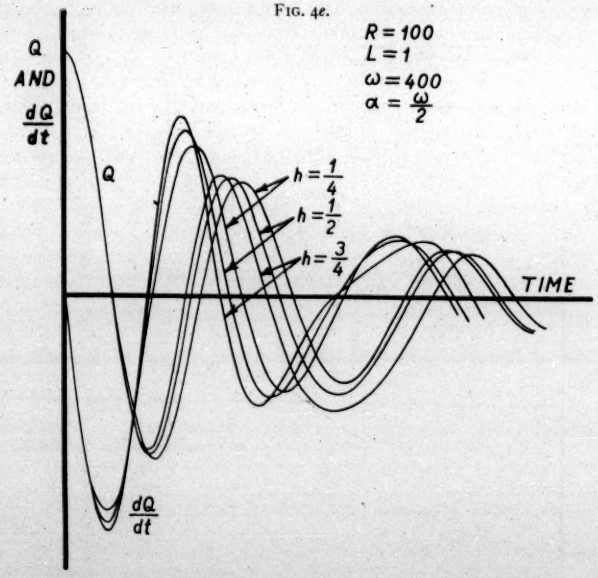


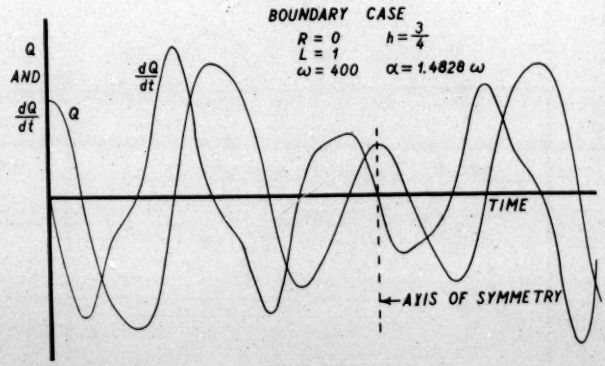
FIG. 4e.



Solutions obtained with the M. I. T. Differential Analyzer. Curves of the instantaneous values of the charge Q and of the current $i = dQ/dt$ for values of R and h as indicated on the respective curves and for the initial conditions: $Q = 1$, $i = 0$ when $t = 0$. These curves show vividly and in a quantitative way just how unstable and stable spontaneous oscillations and the damped oscillations take place in this particular circuit.

the oscillation in the circuit for the specified values of the parameters and for the initial conditions $Q = 1$, $dQ/dt = 0$ for $t = 0$. The curves 4*a*, *b*, *c* show the effect on the current of successively increasing the resistance from zero to 400 ohms when the rate of variation of the capacitance was $\alpha = 2\omega_0$—it will be recalled that $\alpha = 2\omega_0$ produces the most unstable condition. When $R = 100$ ohms, instability, i.e. an exponentially increasing current amplitude, occurs for $h = \frac{3}{4}$ but not for smaller values, while in the case of $R = 400$ ohms, decaying natural oscillations only are obtainable. The curves 4*b*, *d*, *e* show the currents for the three adjustments $\alpha = 2\omega_0$, ω_0, and $\frac{1}{2}\omega_0$, corresponding to the first three unstable regions of Fig. 2, and for a resistance of $R = 100$ ohms. While in all cases the current is increased in magnitude when $h > 0$, it is clear that the only strong effect is that for $\alpha = 2\omega_0$, as previously emphasized. The curves appear to be almost sinusoidal (times an exponential), even the harmonics of low order being relatively unimportant. In Fig. 5, the case is illustrated where the circuit is adjusted exactly to a point of stable spontaneous oscillation; the initial conditions were obtained from Ince's tables.[8] No periodicity is found here, which is directly in accord with the theory [9] of our equation.

FIG. 5.



Illustrating one case of stable spontaneous (undamped) oscillation. The curve was obtained with the M. I. T. Differential Analyzer for initial conditions taken from Ince's tables.[8]

[8] Ince, *Proc. Royal Soc. of Edinburgh*, **45**, 106 (1925).

(*To Be Continued.*)

NOTES FROM THE BARTOL RESEARCH FOUNDATION.

CONTRIBUTION OF THE COSMIC RADIATION TO THE IONIZATION OF THE UPPER ATMOSPHERE.*

BY

THOMAS H. JOHNSON.

On the basis of the earlier measurements of Hess and Kolhoerster, H. Benndorf [1] considered the effect of the cosmic radiation in producing electrical conductivity in the upper atmosphere. In his theory ions were produced at a rate proportional to the product of the cosmic-ray intensity by the density of the atmosphere and they disappeared by recombination, the coefficient for which was determined by the theory of J. J. Thomson.[2] Benndorf concluded that a conductivity 10^{10} times that at the Earth's surface could be produced by the cosmic radiation at 100 km. The increase was due to a hundred-fold increase in the cosmic-ray intensity, accounting for a tenfold increase in the number of free ions and a 10^9-fold increase in ionic mobility.

In recent times cosmic-ray intensities have been measured at elevations greater than those reached by Kolhoerster and considerable evidence has been gained regarding the production of secondaries, giving some rational basis for extrapolation of the ionizing effects beyond the levels attained in balloon-flights. Knowledge of the actual state of ionization in the higher layers has also been greatly enriched by the investigations of radio-wave reflections, following the methods introduced by M. A. Tuve and G. Breit [3] and E. V. Appleton and M. A. F. Barnett.[4] The large diurnal and seasonal

* Reprinted from The Transactions of the American Geophysical Union, 1935, Part 1.

[1] *Physik. Zs.*, 27, pp. 686–692, 1926, and Wien-Harms, "Handbuch der Exper. Physik," Vol. 25–I, p. 279.

[2] *Phil. Mag.*, 47, pp. 337–378, 1924.

[3] *Nature*, 116, p. 357, 1925.

[4] *Nature*, 115, pp. 333–334, 1925.

variations of the reflecting layers lead to the conclusion that most of the ionization is produced by rectilinearly propagated rays from the Sun and the same conclusion was particularly well established by the 1932 eclipse work of S. S. Kirby, L. V. Berkner, T. R. Gilliland, and K. A. Norton.[5] There is, in fact, no difficulty in attributing all of the observed ionization to the ultra-violet radiations from the Sun, although the evidence would not exclude a small but appreciable contribution from some other source. It is thus of interest, in the light of recent developments, to again estimate the amount of conductivity in the high atmosphere which can be attributed to the cosmic radiation.

Though there may exist additional radiations of cosmic origin in the highest layers of the atmosphere, from the experimental point of view the discussion must be limited to those rays which can be studied by direct electroscope or counter measurements in the lower available regions. Typical, and perhaps the best, intensity-measurements of this kind have been made on free-balloon flights by E. Regener.[6] His instruments were thin-walled electroscopes measuring the rate of production of ions in a gas at atmospheric pressure at various heights up to that corresponding to a barometric pressure of 25 mm. The ionizing effect of the same radiation in the ionosphere can be determined by extrapolation of Regener's curves to the point of zero barometric pressure. Three of Regener's flights resulted in curves which flattened out at about 50 mm. to a constant ionization of 300 ions per cc. per sec. per atmosphere. It is thus reasonable to consider this value as representative of the cosmic-ray intensity at all greater elevations, except where changes in the magnetic field must be considered. (Though not specifically stated in Regener's paper, his flights presumably took place from Stuttgart—latitude 49° north and longitude 8° east.)

Before adopting this value it is well to consider certain arguments which have been advanced relative to such an extrapolation. In the first place, if the primary radiation contains photons which contribute to the ionization only after they have produced secondary electrons by collision with

[5] *Bur. Stan. J. Res.*, 11, pp. 829–845, 1933.

[6] *Nature*, 132, pp. 696–698, 1933.

atmospheric matter, it might appear that this process would give rise to a transition-effect in which the ionization would at first increase with depth in the atmosphere. H. Kulenkampff [7] has pointed out that such a transition-effect would not occur in the Earth's atmosphere because of the action of the Earth's magnetic field. This causes the secondary electrons to circulate about their point of origin in circular orbits and, though the rate of production is proportional to the pressure, their duration, or the number of times they circulate about the point of origin, is inversely proportional to the pressure. Hence the number of ions produced by secondary charged particles is independent of the pressure and is proportional to the intensity of the primary radiation. A similar argument also applies to the ionization by secondary particles generated by corpuscular primaries. On the other hand, if primary corpuscles generate secondary photons which in turn generate tertiary particles, the ionization due to the latter would be proportional to the intensity of the secondary photons at every point and would therefore increase, at first, with depth in the atmosphere. A transition of this nature would tend to give a lower ionization in the outer atmosphere than would be indicated by simple extrapolation of Regener's curves.

As a second consideration, the extrapolation over the last 25 mm. of pressure, though a short one on the barometer-scale, is a long one on the scale of geometric distances. Changes in solid angle due to diminished shielding by the Earth may thus be important. Considering straight-line propagation, the solid angle at 100 km. is only 2.8 per cent. greater than at the Earth's surface. On the other hand, high-energy secondaries describing circular orbits would be able to enter the electroscope from nearly twice the solid angle at the higher elevations. This effect would tend to increase the ionization somewhat above that indicated by the simple extrapolation.

A third consideration involves the effect of the greater distance from the magnetic center of the Earth, permitting lower energy-radiations of the corpuscular type to reach the higher levels. At the height of 100 km. the distance from the

[7] *Naturwiss.*, **21**, pp. 25–26, 1933.

center of the Earth is 1.7 per cent. greater than at the surface and the lower limit of the energy of incident radiation at all latitudes is decreased 3.4 per cent. In the latitude of 50° Störmer's theory gives 2.5 billion volts as the lower energy-limit for electrons at the Earth's surface. At the height of 100 km. the lower limit would be 2.4 billion volts. The intensity at the higher level is thus augmented by the slightly greater energy-range.

Though none of the above effects is susceptible of exact calculation, it is reasonable to assume that they do not account for any very great differences between the ionization at 25-mm. and at zero-pressure. The value of 300 ions per cc. per sec. per atmosphere seems thus to be a good estimate of the intensity of the cosmic radiation at all points in the ionosphere.

If q ions are produced per cc. per sec. and the recombination-coefficient is α, the number of ions existing in equilibrium with the ionizing source is

$$n = \sqrt{(q/\alpha)}. \tag{1}$$

In the case of several ionizing sources

$$n = \sqrt{(q_1 + q_2 + \cdots)}/\sqrt{\alpha}. \tag{2}$$

Since (2) is not a linear function of the q's each source of ionization does not contribute independently of the others to the equilibrium-concentration. We define the contribution of the cosmic radiation, n_{c-r}, as the ion-concentration which would exist if no other source of radiation were present. Since $q = Ip$ and $\alpha = \alpha_0 pT/T_0$, where I is the cosmic-ray intensity expressed as the number of ions produced per cc. per sec. in air at one atmosphere and absolute temperature T_0, and α_0 is the recombination-coefficient at a pressure of one atmosphere, it follows that

$$n_{c-r} = \sqrt{IT/\alpha_0 T_0}. \tag{3}$$

Without taking into account the variations of temperature with elevation, and choosing the value of $\alpha_0 = 1.6 \times 10^{-6}$ as given by the theory for atomic oxygen-ions at the temperature of $-60°$ C. prevailing in the stratosphere, the ion-densities are those given in Table 1. Underlying the calculations is

TABLE I.
Ion-densities.

Height, km.	0	20	40	60	80	100
Cosmic-ray intensity, ions/cc./sec./atmosphere	2.7	290	300	300	300	300
Equilibrium-density of atomic oxygen-ions, No./cc.	1.3×10^{3}	1.3×10^{4}	1.4×10^{4}	1.4×10^{4}	1.4×10^{4}	1.4×10^{4}
Equivalent-density of electrons, No./cc.	0.088	0.90	0.92	0.92	0.92	0.92

the assumption that free electrons unite with oxygen-atoms in a time which is short compared with the time of recombination of two ions of opposite charge. The validity of this assumption is well established by the eclipse observations of Kirby, Berkner, Gilliland, and Norton. The equivalent electron-densities, n_{elect}, operative in the reflection of radio waves are related to the ion-densities, n_{ion}, in the ratio of the mass of the electron to that of the oxygen-ion.

$$n_{elect} = n_{ion}\, m_e/m_{ion} = n_{ion} \times 0.34 \times 10^{-4}. \qquad (4)$$

The equivalent electron-densities are in the last row of the table.

Since the critical frequencies of radio waves for which reflection takes place require electron-densities of the order of 10^{5} per cc., it is concluded that the cosmic-ray contribution to the conductivity of the upper atmosphere is negligible compared with that from other sources.

EARTH-MAGNETIC ANALYSIS OF THE PRIMARY COSMIC RADIATION.*

BY

THOMAS H. JOHNSON.

The Earth constitutes a huge magnetic spectrograph which resolves the primary, electrically-charged cosmic rays into a series of band edges, the positions of which depend upon the magnetic rigidity ($H\rho = mv/e$). The rays are deflected in

* Reprinted from The Transactions of the American Geophysical Union, 1935, Part 1, and abstracted from a paper in the *Physical Review*, Vol. 48, pp. 287–299.

vacuous space before they have become confused with the atmospheric secondaries, and the analysis is made from measurements of the intensity of the radiation as a function of the latitude and direction. Variations of intensity from a particular direction with respect to latitude give the distribution with respect to rigidity of the sum of the positively and negatively charged components, and the variations at a particular latitude with respect to meridian-angle give the distribution of the excess radiation of one sign of charge unbalanced by the other. Combining the two sets of experimental data it is thus possible to determine the distribution of rays of each sign of charge individually. Analysis of this type determines only the magnetic rigidity and it is necessary to invoke other methods for determining the mass, charge, and velocity individually. In the usual mass-spectrograph an electric field supplements the magnetic field and enables the determination of e/m independently of v, but in the case of the earth-magnetic analysis of the cosmic radiation atmospheric absorption must be relied upon to give the additional information. The complete analysis thus requires measurements of the intensity as function of direction, latitude, and elevation.

In 1934 measurements of the directional distributions were reported from several stations in equatorial, intermediate, and high latitudes, made with sufficient accuracy to bring out some of the principal features of the east-west asymmetry.[1] When those measurements were analyzed in terms of the theory of the positions of the band edges, it became evident that within the limits of accuracy the positive radiation alone was sufficient to account for all of the variations of intensity with latitude. Thus there was no need to invoke any negative radiation. During the past year progress has been made by Lemaitre, Vallarta, and Bouchaert[2] in developing a more accurate theory, and more extensive and accurate measurements have been realized of the angular distribution of the radiation in the important range of latitudes from 29° to 50° where most of the variations of intensity with respect to latitude take place.

[1] *Trans. Amer. Geophys. Union*, 15th Annual Meeting, pp. 143–147, 1934; *Phys. Rev.*, **45**, pp. 569–585, 1934.

[2] *Phys. Rev.*, **47**, pp. 434–436, 1935.

The present extent of the asymmetry survey is indicated by the map of Fig. 1, the stations being represented by dots. The work in 1933 resulted in reasonably accurate values of the east-west asymmetry at two stations on the geomagnetic equator at depths below the top of the homogeneous atmosphere of 6 and 6.8 meters of equivalent water, at one station geomagnetic latitude 20° at 10.3 meters' depth, and at one station in geomagnetic latitude 50° at 10.3 meters' depth. Less accurate determinations of the east-west asymmetry were also realized at one station on the equator at 10.3 meters' depth and at three stations at 29° ranging in depth from 7.3 t

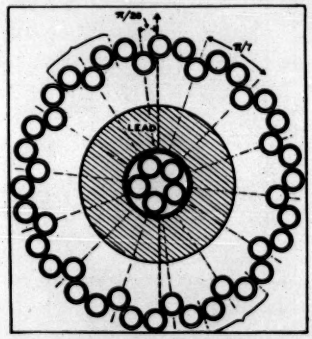
FIG. 1.

10.3 meters. During the summer and fall of 1934 extensive measurements have been realized at four other Mexican stations and at three stations in the United States. Accurate data of the east-west asymmetry were obtained in geomagnetic latitude 29° at atmospheric depths of 10.3 and 6 meters of equivalent water, in latitude 36° at a depth of 8.5 meters, and in geomagnetic latitude 50° at depths of 6 meters, 7.1 meters, and 10.3 meters. Additional studies of the north-south asymmetry were made in latitude 29° confirming some of the newer refinements of the theory of Lemaitre, Vallarta, and Bouchaert. Considerable attention has also been given to the intensity of showers both as a

function of latitude, direction, and elevation, and the results have brought to light some interesting properties of this component of the radiation, which seem to have important bearing on the final analysis of the primary radiation. Studies have also been made of the effect on the east-west asymmetry of the introduction of lead absorbing-screens between the counters, for it is thought that the results of this experiment indicate in what way the measured east-west asymmetries should be corrected to account for scattering.

The apparatus used in the recent work is represented diagrammatically in Fig. 2. Forty-two small counters, con-

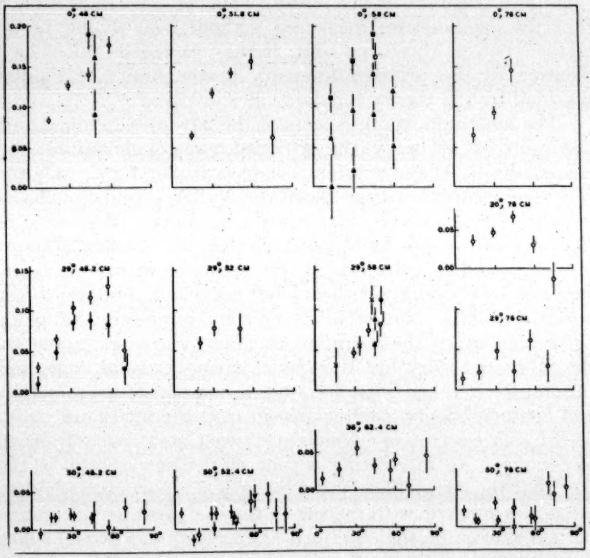
FIG. 2.



nected in parallel groups of three each are arranged on the periphery of a cylindrical chasis and the pulses from diametrically opposite groups are combined with those from a central group of five counters for the selection and recording of the coincidences. The instrument thus combines in one unit seven coincidence-trains, each pointing in a different direction. Automatic changes of position at regular intervals bring each counter-train successively into all of the possible directions and, without calibration as to relative sensitivities or reliance upon long time constancy, comparisons of intensities from all directions can be made. The readings are also recorded automatically.

Figure 3 presents a compilation of all of the east-west comparisons. For each station the ratio of the difference to the average of the west and east intensities is plotted against zenith-angle and the plots are arranged in the order of the geomagnetic latitude and the elevations of the corresponding stations. The principal measurements were made without additional absorbing material but lead blocks of 3.8 cm. and

FIG. 3.



9 cm. were also introduced between the counters with results indicated by the shaded circles. In the case of the lesser thickness a slight increase in the percentage of asymmetry is indicated whereas a slight decrease results from the introduction of the greater thickness of lead. The lead apparently exercises a double effect which may have the following explanation. The softest radiation is somewhat diffused with

respect to the primary directions and its elimination by the thinner absorbing block reduces the uniformly distributed background, producing an increase in the asymmetry. Of the radiations which pass the first layer of the lead, represented by the thinner block, the asymmetrical component is softer than the background of higher energy-radiations and is reduced in correspondingly greater proportion by additional thickness of lead. The effects are important in correcting the measurements for diffuseness with respect to primary directions and deserve further study. The present indication is that the asymmetries measured without lead should be increased two or three per cent. if they are to be taken as a measure of the primary intensity in the limits of rigidity specified by the theory for the angles involved.

The most important aspects of the new measurements are the improved accuracy, the extended range of elevations, and the results for the new intermediate latitude of 36°. During the same interval of time Lemaitre, Vallarta, and Bouchaert have further improved the approximate theory of Störmer of the positions of the band edges. They have taken account of the periodic orbits which are included within the cone specified by the Störmer theory but contribute nothing to the intensity. The cone of infinite orbits intersects the plane perpendicular to the meridian at about the angle given by the Störmer theory but it includes a smaller solid angle and this must be taken into account in comparing asymmetries and latitude-effects, particularly in case the latter are measured by an electroscope which averages the intensity from all directions. An interesting aspect of the new theory is the fact that the cones illuminated by rays of a particular rigidity are not symmetric with respect to the east-west vertical plane but skew towards the equator. This effect was brought into experimental evidence by the measurements of the north-south asymmetry realized in the latitude of 29° at two atmospheric depths, 6 meters and 7.3 meters.[3]

On the basis of the new theoretical results for the range of latitudes from the equator to 20°, new values have been calculated from the asymmetry measurements of the fraction of the intensity due to the unbalanced positive component in the

[3] *Phys. Rev.*, **47**, pp. 91–92, 1935.

range of rigidity below that which cuts off at 60° below the zenith towards the eastern horizon. Account is taken of the absorption of the atmosphere and the results are expressed in per cent. of the total observed intensity. At sea-level 4.9 per cent. of the equatorial intensity and 6.3 per cent. of the intensity at 20° is due to unbalanced positives in the corresponding ranges of rigidity. The difference, 1.4 per cent., is the amount of latitude-effect which can be accounted for by unbalanced positives. At an elevation of 14,000 feet (atmospheric depth of 6 meters) 6.8 per cent. of the equatorial intensity and 8.8 per cent. of the intensity in latitude 20° can be accounted for in the same manner. The difference of 2.0 per cent. is the latitude-effect at this elevation due to unbalanced positives. The accurate sea-level measurements of Millikan and Neher [4] show a latitude-effect of 0.5 per cent. between Peru on the magnetic equator and Panama at latitude 20° so that a sea-level effect of more than the observed amount can be accounted for by positives alone. At an atmospheric depth of six meters the airplane measurements of Bowen, Millikan, and Neher [5] show that the latitude-effect for the same range of latitudes is also very small, possibly not exceeding the 2.0 per cent. attributable to the positive component, though the accuracy is not great enough to exclude a small negative component.

In the lower range of energies, corresponding to the magnetically deviable rays in higher latitudes, the measurements are ready for an extension of the same type of analysis awaiting the development of the theory of the cut-off angles. Without the theory the latitude-effect gives a lower limit for the corpuscular component and the asymmetries show that most of the rays within the observable energy-limits are positive. The asymmetry at the equator can also be used to extend the lower limit set by the latitude-effect. Thus at an elevation of 4,300 meters in high latitudes the 25 per cent. corpuscular component found from the latitude-effect can be augmented by another 5 per cent. because of the asymmetry found at the equator, so that at least 30 per cent. of the intensity at high latitudes is definitely corpuscular. In the

[4] *Phys. Rev.*, **47**, pp. 205–208, 1935.
[5] *Phys. Rev.*, **46**, pp. 641–652, 1934.

same way at sea-level it is found that at least 17 per cent. is corpuscular. Due to the relative softness of the magnetically deviable rays, they occur in greater proportion in the direction of the vertical. From this direction 43 per cent. of the high-latitude intensity at 4,300 meters is definitely corpuscular.

Recent studies of shower intensities [6] have also thrown a new light on the composition of the primary radiation. Table 1 gives the intensities of showers relative to the total

TABLE 1.

Comparison of Shower and Vertical Intensities.

Station.	Geo-magnetic Latitude.	Elevation.	I_s Shower-intensity.	I_v Vertical Intensity.	I_s/I_v.
	°	meters	counts/min.	counts/min.	
Swarthmore, Pa.	50	100	2.2	12.9	0.17
Vera Cruz, Mex.	28	20	2.3	11.9	0.19
Mt. Evans, Colo.	50	4300	15.0	36.8	0.41
Nevado de Toluca, Mex.	29	4300	11.4	28.2	0.40
Copilco, Mex.	29	2280	7.4	22.8	0.32
Parral, Mex.	36	1730	4.6	17.5	0.26

vertical intensities (j_s/j_v) at a series of stations distributed in latitude and elevation. The constancy of the ratio with respect to changes of latitude show that the showers have about the same latitude-effect as the vertical rays and they are therefore produced by an electrically-charged component of the primary radiation. Studies of the asymmetry of the shower-particles presented in Table 2 were made at 4,300

TABLE 2.

Shower Asymmetry on Nevado de Toluca, Elevation 4,300 Meters, Geomagnetic Latitude 29°.

Zenith-angle.	Average Intensity $(I_w + I_e)/2$.	Asymmetry $2(I_w - I_e)/(I_w + I_e)$.
°	counts/min.	
35	3.70	0.0122 ± 0.0114
49	2.50	0.0180 ± 0.0104

meters' elevation in latitude 29° and it was found that the ratio of the difference to the average of the east and west

[6] *Phys. Rev.*, 47, pp. 318–319, 1935.

intensities at two different zenith-angles was very low compared with the asymmetries found for the total radiation so that we are led to believe that the shower-producing primaries consist of equal numbers of positives and negatives. Since the production of secondary photons, now believed to be an essential part of the shower-phenomenon, can be best understood from the theoretical point of view if the primaries are electrons, the combined evidence would seem to indicate that primary positrons and negatrons are also present in the cosmic radiation. It would seem to follow then that the positive asymmetrical component consists of primary protons. Its absorption-coefficient seems to be definitely less than that of the shower-producing radiation so that positive electrons can be eliminated. The heavier positive nuclei are also unlikely candidates because of the low penetrations expected of the higher nuclear charges.

The rate of absorption of radiation included within rather narrow limits of rigidity can be determined from the variation of asymmetry with zenith-angle and elevation. Data of this kind constitute a potential method for analysis of the type of particle involved if it can be shown independently, by theory or experiment, how different types of particles should be absorbed. In the case of the three latitudes 0°, 29°, and 50° the variations of asymmetry with zenith-angle and elevation are given approximately by absorption-coefficients for the magnetically deviable rays of 0.33, 0.36, and 0.53 per meter of water, respectively. The average energy of the asymmetric bands at the same three latitudes are 15, 10, and 3×10^9 volts, respectively. It is a striking fact that the absorption-coefficients vary definitely less than would be indicated by the ratio of the energies. However, we are not as yet in position to apply this finding to the problem of the nature of the primary radiation.

A Numismatic Note.—According to a blue folder issued by the International Nickel Company of Canada, Limited, the oldest coin extant is a Bactrian piece attributed to 235 B.C.; it was hammered from a crude copper-nickel alloy of Chinese origin. A copper-nickel alloy coinage was adopted by the United States of America in 1857 when its copper pennies were minted to contain 88 per cent. copper and 12 per cent. nickel. The present "nickel" or five-cent piece is an alloy of 75 per cent. copper and 25 per cent. nickel.

C.

The Short-Circuit Beetle Et Al.—(U. S. D. A. Clip Sheet No. 908.) California lead-cable borers often damage aerial-telephone and high-tension lines in or near forests by boring holes in the lead sheathing. The holes are bored for the purpose of establishing new homes and not for a variation in diet. Moisture eventually finds its way through the perforated lead covering and short-circuits the electric current. Linesmen for the telephone and power companies call the borer "the short-circuit beetle."

According to entomologists of the Department of Agriculture many other wood-boring insects easily chisel their way through lead and tin signs which block their passage-ways. Curious squirrels sometimes gnaw aluminum identification tags off trees. The ground mole sometimes gnaws into concrete, and cases of rats cutting lead pipes in buildings are not uncommon.

C.

NOTES FROM THE NATIONAL BUREAU OF STANDARDS.*

PENETRATION OF MOISTURE IN MASONRY WALLS.

Rates of penetration of moisture through walls of brick masonry are being measured to determine what combinations of materials and types of construction are most resistant to leakage when exposed to rainfall. The walls are about 50 inches high and 40 inches long, and either 8 or 12 inches thick. Each of the specimens differs from the others with respect to thickness, kind of brick or mortar, or quality of workmanship. Included are walls of each of three kinds of brick, four mortar compositions, and two qualities of workmanship. In the walls of class A workmanship, the joints are well filled with mortar, the method employed by the mason being similar to that described in specifications for high quality masonry. Class B workmanship is intended to be similar to that used in the construction of buildings where inspection is lacking; the interior joints are not filled and the mason does not attempt to use more mortar than is required for an acceptable appearance.

Although the walls are to be tested also under conditions resembling exposure to strong wind and heavy rain, the data available now are largely the results of tests in which the water penetrates under the forces of capillarity and of gravity only. Water is applied by means of a spray at the top of a wall, causing the formation of a continuous film of water on the exposed face of the wall. Observations of the performance of a wall under test include the time for dampness first to penetrate through the wall, the rate of leakage, and the amount of water absorbed during a test.

The results of the tests indicate that, for walls of equal thickness, the quality of workmanship has a major influence on the rate of penetration of water. None of the walls of class A workmanship has leaked, whereas all of those with class B workmanship have allowed water to pass in measurable amounts. When dampness penetrated walls of class A

* Communicated by the Director.

workmanship, the moisture apparently was transmitted by capillarity. With the nearly non-absorptive brick, moisture penetration was more rapid in the mortar than in the brick; with the more absorptive ones it was more rapid in the brick. The greatest leakage occurred with walls of bricks of low absorption and class B workmanship. The data obtained thus far do not show how the composition of the mortar may affect the permeability of walls. They indicate, however, that the quality of the workmanship and the thickness of the walls have a greater influence on permeability than any of the other variable factors under study.

WORKABILITY OF CONCRETE.

The relative ease with which a freshly mixed concrete may be placed in a uniform and compact state in the forms is of much importance from an economic and technologic viewpoint in the construction of concrete buildings and engineering structures. The property which is related to ease of placement is known to the engineer as the workability of the concrete. There has been no exact definition of the workability of concrete and there has been a current impression that the workability could be measured by a single test method and expressed by a single numerical value.

The Bureau is now engaged in a study of methods of measuring the workability of concrete. The present study has indicated that workability is dependent upon several independent factors, and tentative test methods have been developed for measuring its harshness, potential segregation, shear resistance, and adhesion. The harshness test distinguishes between the smoothness of a concrete with excess sand compared to the roughness of one that is deficient in sand; the potential segregation measures the mortar that separates from the concrete when it is jolted; the shear resistance or the resistance to flow which will, for example, resist the leveling of a heaped batch of concrete due to the action of gravity; the adhesion, which measures the "stickiness" of the cement.

The test methods were applied to concretes made with several cements. The mixes covered a wide range of aggregate grading. The amount of cement and of water in the

mix, the grading of the aggregate, fineness of the cement, etc., were found to influence the workability factors. The influence of these variables was different for the different tests; for example, the amount of cement present in a given quantity of concrete markedly affected the adhesion but did not appreciably affect the segregation; however, the ratio of cement to sand in the concrete did materially affect the segregation.

The results of the investigation will shortly appear in the Proceedings of the American Concrete Institute.

CHEMICAL COMPOSITION OF PORTLAND CEMENT.

Recent work at this Bureau and elsewhere has emphasized the heterogeneous nature of portland cement by showing that its chemical constituents are distributed unequally among the differently sized particles making up the whole cement. Recognition of the existence of these inequalities in the size distribution of the chemical constituents is important since the inequalities are sufficiently large in many cases to render futile any attempts at quantitative explanation of the behavior of the whole cement merely on the basis of its chemical analysis. A further study of the problem on a larger number of cements has been made at the Bureau during the past year and the data found to support and extend previous conclusions.

In all, thirteen cements, representing products of six different manufacturers, have been separated into size fractions and analyzed. The size separations were made by air elutriation as described in *J. Research N. B. S.*, **14**, 419 (1935); RP777.

The results of the chemical analyses of the fractions showed that, in general, the tricalcium silicate in a cement tends to be more concentrated in the finer sizes, while the greatest percentages of dicalcium silicate are found in the largest sizes of particles. The tetracalcium alumino ferrite and the tricalcium aluminate seemed to be about equally distributed between the fine and coarse particles. The 0–7 micron fraction differed from the others in that it had the highest ignition loss and also contained the greatest portion of the sulphuric anhydride.

Five of the thirteen cements were selected for more extensive study in the hope of arriving at an explanation of certain known discrepancies in the relation between their chemical compositions and some of the physical properties of these particular cements. These five cements were of very similar chemical compositions and yet they were known to have rather widely differing strength and heat of hydration characteristics. It was believed that these discrepancies might have been brought about by the unequal size distributions of the chemical constituents, and heat of hydration measurements were accordingly made on the individual fractions in order to find out if any better correlations could be obtained for definite sizes of particles of known composition.

Several equations were derived by the method of least squares giving the heats of hydration of one size fraction for one age as linear functions of the four major compounds, but none of these equations were found to yield good correlations, and the constants derived did not appear to be such as to have any physical significance. This suggested that if the properties of these cements were to be fully explained, further studies of the chemical constitutions would have to be made and that possibly some of the observed peculiarities were the result of errors in the compound compositions (calculated by the Bogue method) brought about by incomplete crystallization of the cement clinkers. Such effects have recently been pointed out by Lea and Parker in England.

Petrographic examinations on etched thin sections of the cement clinkers supported the theory that the calculated compositions were in error. The compound compositions, as estimated petrographically, differed considerably from the calculated values and some of the clinkers were found to contain appreciable quantities of glass. Furthermore, when the heats of solution of the whole cements were calculated from their compound compositions as estimates petrographically, better agreement with the observed values resulted than when the heats of solution were calculated from the compound compositions by the method of Bogue. It thus appears that poor correlations obtained between the physical and chemical properties of cements and their calculated compound compositions may be due to inaccuracies in the method of computing the compound compositions.

SILVER COATINGS TO PROTECT CARBON STEELS DURING ANNEALING.

In the course of some recent work at the Bureau, which required the full annealing of both medium carbon and high carbon steels with the utmost protection against oxidation or decarburization of the surfaces, it developed that silver coatings are a satisfactory means to accomplish this end, although probably not so well known or so widely used as are other methods of protection.

Polished mild steel surfaces bearing finely engraved designs may be coated with a thin electrolytic deposit of silver, annealed, and the silver stripped electrolytically, leaving the steel surface in practically its initial condition. Electroplated silver has also been used to protect rather thin section, high-carbon steel pieces during annealing with assurance that the insolubility of silver in iron will prevent modification of the chemical composition of the steel by penetration of the silver.

STORAGE OF LINEN WITH CELLULOSE NITRATE KNITTING NEEDLES RESULTS IN DAMAGE TO CLOTH.

Textile laboratories are often called upon to determine the cause of deterioration in or damage to textiles from an examination of the damaged material. This may be a rather difficult problem because the undamaged material is seldom available for comparison and valuable evidence is often destroyed by laundering or use of the textile subsequent to the occurrence of the damage.

An interesting example of deteriorated linen was brought to the textile section of the Bureau recently. This linen had been kept for a number of years in the drawer of a sewing cabinet located in a living room. One piece had been hemmed and was ready for use as a napkin. The napkin, apparently in good condition, was used and sent to the laundry. When it came back there were large, symmetrically spaced holes along the folds. An unobservant person might have held the laundry responsible for this damage, but the location and nature of the holes showed clearly that the damage was done while the napkin was folded. No chemicals were found in the fabric which would account for the damage.

Other pieces of the linen which had not been laundered, were found to be greatly deteriorated. In some places they were brittle and would powder when rubbed between the fingers. Several knitting needles stored in the same drawer were likewise found to be weak and brittle.

The last mentioned observation was the clue to the solution of the problem. The knitting needles were found to be made of a cellulose nitrate plastic. Cellulose nitrate is not very stable. It liberates oxides of nitrogen when it deteriorates. The linen was strongly acid in reaction and gave a strong test for nitrate ion.

The evidence seems conclusive that nitric acid formed from the gases given off by the decomposition of the cellulose nitrate knitting needles attacked the linen over a period of time and was the cause of the damage. Nitric acid was not found in the laundered napkin because it had been removed in the laundering.

If the owner of the deteriorated linen had not been observant and had not furnished all of the material bearing on the problem, the cause of the deterioration probably would have remained a mystery.

SOME PHYSICAL PROPERTIES OF ISOPRENE.

Pure isoprene has been prepared for the purpose of measuring its entropy and free energy and thereby determining its thermodynamic relation to rubber. The isoprene was made by the cracking of dipentene and was purified by three successive distillations through an efficient 40-bulb fractionating column. A fraction boiling within a range of about 0.01° C. was obtained. The purity of this fraction was of the fifth (or highest) order on the scale of Swietoslawski as determined by the difference, 0.002° C., between the temperatures of boiling and condensation. Some of the physical constants of this fraction are as follows:

Boiling point (° C.)	34.076
Change of boiling point with pressure (° C. per mm.)	0.0383
Freezing point (° C.)	about − 146
Density at 20° C. (g. per ml.)	0.6805
Refractive index, n_D^{20}	1.42160
Change of refractive index with temperature (per ° C.)	0.00066_3
Molecular refraction (from Lorenz-Lorentz formula)	25.39

When the isoprene was stored at a temperature of about 8° C. over a period of days it was observed to undergo a progressive change as indicated by increases in density, refractive index, boiling point, difference between boiling and condensation temperatures, and rate of change of boiling point with pressure. This change is doubtless due to polymerization and will necessitate the use of freshly distilled samples in the determinations of heat capacity and heat of combustion from which the thermodynamic constants are to be derived.

New Uses for Hollow Glass Bodies.—DIPL.-ING. GERHARD EHLERS, V. D. I. (*Engineering Progress*, Vol. XVI, No. 12.) With the progressive development of its chemical, physical, and strength properties, glass has become a material capable, in many cases, of enabling the engineer to effect essential improvements in the design of his apparatus and machinery. Investigations undertaken by the Berlin Municipal Electricity Works showed that enamel of kitchenware burst off under impact blows while no damage resulted to glass tubes under the same blows. Other properties favor the use of special glass for a considerable field. Glass is now used for electrical hot water cisterns and hot water pipe lines. Glass beer lines have been installed both at Jena and in Berlin. As is well known the taste of beer is decisively influenced by the cleanliness of piping and the glass lines greatly facilitate surveillance. Special Jena or special Orsam glass is used for this purpose. In Berlin, a trial has been made with a glass tube cooling coil in a bar. In spite of the lower thermal conductivity of the glass, no sensible change in the cooling effect was observed. More extensive glass pipe lines, in some instances of a length of over 300 ft. have for some time been in operation in breweries. In some branches of the chemical apparatus industry, glass appliances of considerable dimensions have been developed for use under conditions of exposure to particularly severe demands in the way of chemical and thermic stability. High duty evaporators of over 1100 sq. ft. evaporating area and a total height of 20 ft. have been built from glass, glass tubing being largely used for connecting the various members of the apparatus.

R. H. O.

THE FRANKLIN INSTITUTE

STATED MONTHLY MEETING, FEBRUARY 19, 1936.

The regular monthly meeting of the Institute was called to order at 8:15 o'clock by Mr. Nathan Hayward, President. It was announced that the minutes of the previous meeting had been printed in the February number of the JOURNAL of the Institute, and if there were no objections or corrections, the minutes would be approved. Upon hearing none, the President declared them approved as printed.

The Secretary then reported the following additions to membership since the last meeting: Institute members—76, Museum members—422, total—498.

The chairman then called upon the Secretary to read the appointments of the standing Institute committees for 1936, which are as follows:

Committee on Library.

Mr. H. Bruce Anderson	Mr. J. G. R. Hecksher
Mr. Clifford W. Bates	Mr. L. H. Hendrixson
Mr. William G. Ellis	Dr. Joseph S. Hepburn
Mr. George S. Gardner	Mr. Lionel F. Levy
Mr. F. Lynwood Garrison	Dr. Max Trumper

Committee on Meetings.

Dr. James Barnes	Mr. Charles Penrose
Dr. George S. Crampton	Mr. Coleman Sellers 3rd
Dr. T. G. Delbridge	Dr. W. F. G. Swann
Mr. W. H. Fulweiler	Mr. James G. Vail
Dr. A. W. Goodspeed	Mr. W. C. Wagner

The following gentlemen, recipients of the Franklin Medal, were proposed for Honorary Membership: Dr. Frank Baldwin Jewett of New York City, and Mr. Charles Franklin Kettering of Detroit, Michigan. On motion duly seconded, they were unanimously elected.

Announcement was made of the change in library hours. The reading room and library will hereafter be open on Wednesdays and Thursdays from two until ten o'clock P.M.

The chairman then requested Dr. W. F. G. Swann to take charge of the meeting. Dr. Swann then introduced the speaker of the evening, Dr. K. Lark-Horovitz, Director, Physical Laboratory, Purdue University, Lafayette, Indiana, who delivered an interesting address on "The Application of X-Rays to the Analyzation of the Structure of Wood of Violins and Other Stringed Instruments."

The speaker gave an interesting account of his work in the use of x-rays for the determination of changes in and damage to stringed instruments. He reviewed the acoustical investigations which have been made to determine an objective scale for defining quality of tone. He gave consideration also to the

influence of shape and mechanical properties on tone qualities, and stressed the importance of the choice of material as a determining factor. He discussed the relation between x-ray findings and investigations of other types, and referred to the work of F. Savart, about a century ago, in this field. He described a new simple method for determining the fiber in wood, an important factor in woods for stringed instruments.

The subject was illustrated by photographic prints, illuminated radiographs of violins, and lantern slides. At the close of the paper a lengthy and spirited discussion ensued.

Dr. Swann conveyed the thanks of those present to the speaker, after which the meeting adjourned.

HENRY BUTLER ALLEN,
Secretary.

COMMITTEE ON SCIENCE AND THE ARTS.

(*Abstract of Proceedings of Stated Meeting held Wednesday, February 12, 1936.*)

HALL OF THE COMMITTEE,
PHILADELPHIA, FEBRUARY 12, 1936.

MR. CHARLES H. MASLAND, 2D, *in the Chair.*

The following reports were presented for first reading:

No. 3006: "Speedy" Moisture Tester.
No. 3012: Clark Medal.
No. 3013: Literature.

GEO. A. HOADLEY,
Secretary to Committee.

LIBRARY NOTES.

RECENT ADDITIONS.

American Radio Relay League. The Radio Amateur's Handbook. The Standard Manual of Amateur Radio Communication. 1936.

American Society for Testing Materials. Proceedings of the Thirty-eighth Annual Meeting. Volume 35, Parts 1–2. 1935.

BAUER, O., H. ARNDT, AND W. KRAUSE. Chromium Plating with Special Reference to its Use in the Automobile Industry. Translation from the German by E. W. Parker. With Introductory Chapter on Electroplating Practice and the Properties of Chromium and Nickel Coatings by A. W. Hothersall. 1935.

BRAUNMÜHL, H. J. VON, AND WALTER WEBER. Einführung in die angewandte Akustik insbesondere in die neueren Probleme der Schallmessung, Schallübertragung und Schallaufzeichnung. 1936.

Bulletin Almanac and Year Book. 1936.

CAMPBELL, PERCY A. Generation of the Universe and "Design for Living." 1934.

Chemiker-Kalender, 1936.

Chemisches Zentralblatt. Generalregister VIII über die Jahrgänge 1930–1934. Teil I: Autorenregister, Teil II: Patentregister. 1935.

CLARK, WALLACE. The Gantt Chart. A Working Tool of Management. 1935.

COLVIN, FRED H., AND FRANK A. STANLEY. Turning and Boring Practice. Modern Machines, Tools and Methods Used in Representative Plants. First Edition. 1936.

COWDREY, IRVING H., AND RALPH G. ADAMS. Materials Testing, Theory and Practice. Second Edition. 1935.

DAWES, CHESTER L. A Course in Electrical Engineering. Volume I: Direct Currents, Second Edition, 1927; Volume II: Alternating Currents, Third Edition, 1934.

Deutsche Lichtbild. Jahresschau 1936.

ELLIS, CARLETON. The Chemistry of Synthetic Resins. 1935.

FRANCIS, F. Notes on Organic Chemistry. 1935.

Geologisches Zentralblatt. Anzeiger für Geologie, Petrographie, Palæontologie und verwandte Wissenschaften. Sachlich geordnetes Inhaltsverzeichnis zu Band 31–50. 1935.

GHIARARDI, ALFRED A. Radio Physics Course. An Elementary Text Book on Electricity and Radio. Second Edition, Revised and Enlarged. 1933.

GLUUD, W. International Handbook of the By-Product Coke Industry. American Edition by D. L. Jacobson. 1932.

GRIMSEHL, E. A Textbook of Physics. Volume V: Physics of the Atom. 1935.

HAAS, ARTHUR. Die Umwandlungen der chemischen Elemente. 1935.

Jane's Fighting Ships. 1935.

JONES, H. SPENCER. World's Without End. 1935.

KIMBALL, DEXTER S., AND JOHN H. BARR. Elements of Machine Design. Third Edition, Thoroughly Revised and Enlarged. 1935.

Klimschs Jahrbuch des graphischen Gewerbes. 29 Band. 1936.

LEE, RICHARD E. Man the Universe Builder. Backgrounds and Foundations of the Scientific Technique. First Revised Edition. 1935.

Lincoln Electric Company. Procedure Handbook of Arc Welding Design and Practice. Third Edition. 1935.

LOCKWOOD, MARIAN, AND ARTHUR L. DRAPER. The Earth Among the Stars. 1935.

METCALF, LEONARD, AND HARRISON P. EDDY. American Sewerage Practice. Volume III: Disposal of Sewage. Third Edition. 1935.

MILLER, SAMUEL C., AND DONALD G. FINK. Neon Signs. Manufacture-Installation-Maintenance. First Edition. 1935.

MOULTON, FOREST RAY. Consider the Heavens. 1935.

Penrose Annual Review of the Graphic Arts. Volume XXXVIII. 1936.

PERRET, FRANK A. The Eruption of Mt. Pelée 1929–1932. Carnegie Institution of Washington. Publication No. 458. 1935.

RADLEY, J. A., AND JULIUS GRANT. Fluorescence Analysis in Ultra-Violet Light. Second Edition. 1935.

RÂY, PRAFULLA CHANDRA. Life and Experiences of a Bengali Chemist. Volume 2. 1935.

RUDDIMAN, EDSEL A., AND ADLEY B. NICHOLS. Imcompatibilities in Prescriptions for Students in Pharmacy and Medicine, Practicing Pharamcists and Physicians. Sixth Edition, Rewritten and Reset. 1936.

SAHA, M. N., AND B. N. SRIVASTAVA. Treatise on Heat (Including Kinetic Theory of Gases, Thermodynamics and Recent Advances in Statistical Thermodynamics) Being the Second and Revised Edition of Text Book of Heat. 1935.

SCHWARZ, E. W. K., AND HERBERT R. MAUERSBERGER. Rayon and Synthetic Yarn Handbook. Second Enlarged Edition. 1936.

Thomas' Register of American Manufacturers—1936. Twenty-sixth Edition. 1935.

THOMSON, J. ARTHUR. Biology for Everyman. Two volumes Edited by E. J. Holmyard. 1935.

WATSON, FLOYD ROWE. Sound. An Elementary Textbook on the Science of Sound and the Phenomena of Hearing. 1935.

Who's Who, 1936.

World Almanac and Book of Facts for 1936.

BOOK REVIEWS.

AMATEUR TELESCOPE MAKING, edited by Albert G. Ingalls, Associate Editor, *Scientific American;* with a foreword by Dr. Harlow Shapley, Director, Harvard College Observatory. 499 pages, illustrations, 28 × 32 cms. New York, Munn and Co., 1935, Fourth Edition.

When a technical book passes through four editions in less than ten years, and when that book has been accepted as a veritable bible in the field which it covers, the reviewer's usual complimentary remarks seem entirely superfluous. The latest edition of "Amateur Telescope Making" meets in every way the high standard set by its predecessors. Its plan is the same—a series of very practical articles by such men as Russell W. Porter, Rev. W. F. A. Ellison, Dr. George Ellery Hale, and others, who are able to tell the beginner of the troubles which they encountered, and overcame, and so help him to avoid falling into them again. According to a note by the editor, the new edition "contains new sections on mountings, HCF laps, drives, machine polishing, sidereal time, replacing older sections; also some new notes and corrections."

Fortunately, this edition came out just in time for use by the members of the Amateur Telescope Makers' Section of The Franklin Institute. Ever since the early plans of the Institute's new building, there has been a space, partly over the Fels Planetarium, assigned to this group, and on the 31st of January it was finally opened. Here are provided places to work, as well as materials and lockers, for those who want to grind mirrors for home-made reflecting telescopes, and on four evenings a week expert instructors are showing the beginners just how to do it. And this is just one of dozens of similar groups in many parts of the country, all of them an outgrowth of the original organization of the Telescope Makers of Springfield, Vermont, founded by Russell Porter, and the inspiration of this book. To the members of such groups it has become an essential, and it is hard to imagine anyone foolish enough to start making a telescope mirror without having a copy by his (or her, for telescope making is not solely a masculine hobby) side. This reviewer would like to express the hope that he may be able to record many future editions, and that they may continue to aid in building telescopes through which the owners can learn more about their home, the Universe!

JAMES STOKLEY.

MAN THE UNIVERSE-BUILDER, Backgrounds and Foundations of the Scientific Technique, by Richard E. Lee, A.M., M.Sc., Sc.D., Head of the Department of Chemistry, Allegheny College. 443 pages, illustrations and tables, 14.5 × 22.3 cms. Baltimore, The Williams & Wilkins Company, 1935. Price $3.00.

All of us in our everyday life use the products of scientific developments. As a matter of fact, the use of them has become so routine a matter that we are prone to forget that back of it all is a motive power that has urged man to seek further and further into the realm of the unknown. Many of us are workers or students of a particular technical phase of science, both in research and the application of science, but few of us have had presented a view of science as a whole, to really understand just what its meaning is. Truly, this is a subject of culture giving a promise of untold enlightenment for the mind. Few books have been written on this subject in as clear and complete a manner as the present volume.

In the foreword of the book the author explains that newer educational programs may be traced to broader viewpoints comprising an integration of all the special sciences in order to obtain a unity, which is essential to any appreciation of it. In an effort to meet the requirements of these newer educational procedures with respect to a textbook for a course designed to orient the individual with respect to science the author was led to the publication of The Backgrounds and Foundations of Modern Science and owing to the method employed, the publication of the present book, an abridged edition of the aforesaid book, was made.

There are five parts to the book. The first, an introductory, explains what is meant by the Universe Builder—all men except those biologically and intellectually "still up a tree"—and under a statement of objectives it is stated that since it may be regarded that in the present scientific civilization a knowledge of science is essential, a preliminary view of the nature of man and his environment is made in this light, that one may attain a rational synoptic outlook on man and his universe. In putting forward this thought the author makes quite plain defects in some educational procedures and the purpose of the proposed orientation courses.

From here the reader is taken through parts on the origin of the scientist and the philosopher; basic principles in the method of science in which particular interest may be centered on the treatment of the principle of causation, relation of cause and effect and the laws of nature; the limitations of science wherein there is discussed limitations imposed by the sense organs, symbolical representations, methods, instruments etc. Under the heading of "Development of Modern Scientific Techniques" there is a considerable portion devoted to the universe in which a conception of the whole is attained by building up the various parts i.e. the universe of common sense experience, of science, etc. and orderliness, the space-time concept and others. This chapter should be followed slowly and with great care. Living systems and thinking systems are discussed as well as evolution, the origin of the solar and earth-moon systems and geological and organic evolution which lead up to what might be termed an apex, the mechanism of man's approach to the universe.

Part IV is a very interesting and factual treatment on historical backgrounds of man's techniques, and the final part of the book is a special study of various

aspects of the "primary stuff" of the universe—radiation, the new electromagnetic spectrum, the quantum theory.

The general reader will find much of interest in this book presented in an inspiring way that leads to a more serious contemplation of it. To actually absorb the contents in its entirety requires considerable time, thought and study for the average reader. The results, however, should prove invaluable.

R. H. OPPERMANN.

SHORT WAVE RADIO HANDBOOK, by Clifford E. Denton. 127 pages, illustrations, tables, 16.5 × 21.7 cms. New York City, Radio and Technical Publishing Co. Price $1.00.

The author states in his introduction that this book represents a collection over a period of several years of useful information obtained from his files that may prove of advantage to amateur experimenters. Taking this into account one must expect an unusual presentation of material, even though consideration is given to editing and the presenting of a successive procession of subjects. A feature in this handbook is that there is no subject index, a thing commonly associated with similar standard works. The book is truly what the author states, a collection of practical data from his files for amateur experimenters. It deals with the reception of signals and voice only.

The opening chapters are devoted to phones and speakers, and carried on through audio amplifiers, detectors, radio-frequency amplifiers, antennas up to power supply units. It is stated that this sequence has been done with a definite object in mind: to convey to the experimenter the idea that he should start at some definite point and work backwards until the equipment chosen will give the desired service. Following this there is information and explicit directions for the construction of various kinds of receiving sets, and a short wave meter.

The book throughout contains a mass of useful data in the form of tables and diagrams, some of it not readily found in other books of this type. After having gone through the book once, it should not be difficult at a later time to locate desired data.

The book should be of special value to all those hobbyists and experimenters of the era of the 1920's who derived much pleasurable pastime in constructing their own radio receiving sets and who wish now to enter the much more fascinating field of short wave radio reception.

R. H. OPPERMANN.

EINFÜHRUNG IN DIE ANGEWANDTE AKUSTIK, INSBESONDERE IN DIE NEUEREN PROBLEME DER SCHALLMESSUNG, SCHALLÜBERTRAGUNG UND SCHALLAUFZEICHNUNG, von Dr. H. J. von Braunmuhl und Walter Weber. 216 pages, illustrations, tables, 15.8 × 23.5 cms. Leipzig, S. Hirzel, 1936. Price 9.20 marks.

Acoustics is another outgrowth of scientific progression, founded on fundamental physics, which has come to the fore within the last two decades. That it is rapidly becoming necessary for an increasing number of technicians to be familiar with this subject, can be seen on almost every hand, with new and more numerous applications. To meet this need there have appeared books treating on acoustics from different angles and different viewpoints.

The book at hand has as its basis the physiological aspect of the subject—response of the ear drums to sound waves—and physics fundamentals which include wave mechanics. Upon this is built the treatments that follow, microphones and loud speakers, the procedure for sound measurement and the procedure for drafting or outlining sound. The treatment of the latter topic is of particular interest because of the simple manner in which it is presented. The part treats separately the mechanical method, the light-electric method, and the magnetic method.

The remainder of the text is devoted to the acoustics of rooms, acoustics with relation to building construction and construction materials, and the transfer of sound. Throughout the book are photographs of apparatus, curves and sketches, wherever necessary to facilitate understanding. References are recorded almost on every page to works of others on the subject. A subject index completes the book.

This book is well worth while for study with a view to becoming acquainted with the technical fundamentals of the subject.

R. H. OPPERMANN.

HANDBOOK OF THE HEAVENS. Edited by Hubert J. Bernhard, Dorothy A. Bennett and Hugh S. Rice. Foreword by Harlow Shapley. Sponsored by The American Museum of Natural History. 131 pages, 15 × 23 cm. 87 Illustrations. Whittlesey House, New York, 1935. Price $1.00.

There is scarcely any lack of guides to the heavens, but it is a pleasure to welcome the present attractive and well built addition.

To fulfill its purpose as "A Simple Introduction to the Study of the Stars" charts of the autumn, winter, spring and summer skies based on latitude 40° north are given together with charts for the northern and southern regions. Brief descriptions of important features of the several constellations are presented, with helpful suggestions for observers possessed of glasses or small telescopes. Smaller diagrams supplement the seasonal groups.

Maps showing the courses of the planets good until July, 1936 are an interesting feature. Lunar geography is well treated with the help of charts and photographs. Meteors, comets, double stars, solar observations, nebulæ, variable stars follow in turn with considerable direction on means of viewing with or without small telescopes. Seven pages of hints on telescope usage, six on amateur photography, further notes, a suitable glossary and an index complete the volume.

Helpful services which may be rendered by amateurs are cited, notably with respect to meteors and variable stars. This work is headed up through the American Meteor Society, Upper Darby, Pa., and the American Association of Variable Star Observers, Cambridge, Mass.

Well printed on good paper, bound in blue cloth, suitably illustrated and provided with precise charts (some unfortunately now out of date) this forms a practical out-of-door guide to many interesting experiences.

LESLIE R. BACON.

PUBLICATIONS RECEIVED.

Foundations of Physics, by Robert Bruce Lindsay, and Henry Margenau, 537 pages, tables, illustrations, 15.5 × 23.5 cms. New York, John Wiley and Sons, Inc., London, Chapman and Hall, Ltd., 1936. Price $4.50.

Graphical Solutions, by Charles O Mackey, 130 pages, tables, illustrations, 15 × 23.4 cms. New York, John Wiley and Sons, Inc., London, Chapman and Hall, Ltd., 1936. Price $2.50.

Alternating-Current Machines, by A. F. Puchstein and T. C. Lloyd, 582 pages, tables, illustrations, 15.5 × 23.5 cms. New York, John Wiley and Sons, Inc., London, Chapman and Hall, Ltd., 1936. Price $5.00.

Phenomena in High-Frequency Systems, first edition, by August Hund, 642 pages, tables, illustrations, 15.5 × 23.5 cms. New York and London, McGraw-Hill Book Company, Inc., 1936. Price $6.00.

Sulfuric Acid Manufacture, by Andrew M. Fairlie, 669 pages, tables, illustrations, 15.7 × 23.4 cms. New York, Reinhold Publishing Corporation, 1936. Price $9.75.

Hot Springs of the Yellowstone National Park, by E. T. Allen and Arthur L. Day, 525 pages, plates, tables, illustrations, 23 × 29.5 cms. Washington, Carnegie Institution of Washington, 1935.

International Hydrographic Bureau, Year-Book, 1936, 85 pages, 13.8 × 18.7 cms. Monaco, Hydrographic Bureau, 1936.

Bell Telephone Laboratories, Monographs: B-891, An Electromechanical Representation of a Piezoelectric Crystal, by W. P. Mason, 12 pages, illustrations. B-892, Earth-Potential Measurements During the International Polar Year, by G. C. Southworth, 18 pages, tables, illustrations. B-897, A Note on the Source of Interstellar Interference, by Karl G. Jansky, 6 pages, illustrations. 3 pamphlets, 15.5 × 23 cms. New York, Bell Laboratories, 1935.

National Advisory Committee for Aeronautics, Technical Notes: No. 550, Limitations of the Pilot in Applying Forces to Airplane Controls, by M. N. Gough and A. P. Beard, 13 pages, tables, illustrations, plates. No. 551, Tank Tests of Three Models of Flying-Boat Hulls of the Pointed Step Type with Different Angles of Dead Rise—N. A. C. A. Model 35 Series, by John R. Dawson, 11 pages, plates, tables. No. 552, Wind-Tunnel Tests of Wing Flaps Suitable for Direct Control of Glide-Path Angle, by Fred E. Weick, 5 pages, plates. No 553, Notes on the Technique of Landing Airplanes Equipped with Wing Flaps, by Melvin N. Gough, 9 pages, plates. 4 pamphlets, 20 × 26.5 cms. Washington, Committee, 1936.

Canada Dominion Bureau of Statistics, Fisheries Statistics of Canada, 1934, 285 pages, tables, illustrations, 16.5 × 24.5 cms. Ottawa, King's Printer, 1935. Price 35 cents.

CURRENT TOPICS.

Coal Hydrogenation at Billingham.—The London Correspondent of *Chemical and Metallurgical Engineering* in its December 1935 number describes the first commercial scale hydrogenation plant for bituminous coal, that of the Imperial Chemical Industries' at Billingham-on-Tees, England. The plant is evidence that it is technically possible for the country to become self-sufficient as regards petroleum products, with the present exception of certain lubricating oils. When the plant reaches full production it will produce 30,000,000 imperial gallons of gasoline annually from coal with an additional 15,000,000 gal. from various tar products. The process is capable of turning 60 per cent. by weight of the clean coal processed into a high quality gasoline, but is sufficiently flexible to produce also fuel oil, diesel oil, and liquefiable hydrocarbon gases such as propane and butane. It requires only coal and water as raw materials. Hydrogen for the process is made by the decomposition of steam by means either of coke or the residual hydrocarbon gases from the process. Raw coal is ground with oil previously made in the process to a 50 per cent. coal-in-oil "paste." This paste is passed through high pressure injecting apparatus which pumps it up to the conversion pressure. It is mixed with hydrogen and the mixture heated to the reaction temperature. Liquefaction of the coal takes place at 850 deg. F. and 250 atmospheres pressure. The products obtained are subjected to various treatments. The coal is thus converted into a small solid, consumable residue, gas and gasoline.

R. H. O.

A New Phase Rotation Indicator.—A. R. HAND (*General Electric Review*, Vol. 38, No. 12). Electrical engineers well know that phase rotation in polyphase circuits must be determined in order to predict the direction of rotation of polyphase motors for machine drive, elevators, air conditioning equipment etc., and to determine the proper connections for paralleling generators, transformer banks and for many other applications. A considerable variety of phase rotation indicators have been known and used in the past. A new indicator has been developed which makes use of a small capacitor connected in Y with two small neon lamps across the three phase circuit to be tested. An unequal distribution of voltages in the three arms of the Y network occurs and the voltage across one

neon lamp becomes considerably greater than that across the other for a given phase rotation. Voltage across the glowing lamp leads that across the dark lamp. The indicator has the advantages that it is entirely static, having no moving parts, bearings, or pivots; it is sturdy and contains no fragile filaments; it gives positive and unmistakable indications; it will not burn out on moderate accidental overvoltages; it has long life and consequent reliability; it is applicable to 115-, 230-, or 460-volt circuits, at either 60 or 25 cycles; it is small, light, and low in cost.

R. H. O.

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